ENDEAVOUR

A quarterly review designed to record the progress of the sciences in the service of mankind

VOLUME VI JULY 1947 NUMBER 23

Louis Pasteur

The fiftieth anniversary of the death of Louis Pasteur fell on 28th September, 1945, but at that time scientists of the world were too preoccupied with the immense task of reorganizing their work after six years of war to mark the occasion with the ceremony it deserved. Although Pasteur's researches were to a great extent in the fields of medicine and biology he was first and foremost a chemist and made many notable chemical discoveries, especially in the field of stereochemistry. His successes in biology and medicine were due in large measure to his application of chemical methods to these branches of science—a task which an earlier great French chemist, Lavoisier, would doubtless have essayed but for his untimely death. The present is a particularly appropriate time to recall Pasteur's life and work, for the centenary celebrations of The Chemical Society and the Eleventh International Congress of Pure and Applied Chemistry have at this time brought to London the greatest gathering of chemists that the world has ever seen.

Louis Pasteur was born at Dôle (Jura) on 27th December, 1822. His father was a tanner, who, appreciating the value of education, sent his son to be educated at Besançon, and in 1843 Pasteur was admitted to the *École Normale* in Paris. It was while working as an assistant in the laboratory of A. J. Balard at the *École Normale* that he made the fundamental discovery of molecular asymmetry, in 1848. In the same year he went to Dijon, and soon afterwards to Strasbourg, as a teacher. In 1854 he became professor and dean of the faculty of sciences at Lille, and in 1857 he returned to the *École Normale*, as director of scientific studies.

When he began his observations on tartrates in 1847, it was known that certain organic compounds in the liquid or dissolved state were capable of rotating the plane of polarization of polarized light, but no satisfactory explanation of the phenomenon had been advanced. Pasteur not only showed that the effect on polarized light was due to asymmetry of molecular architecture, but devised methods of resolving racemic mixtures. Indeed, his three methods of resolution—mechanical separation, separation by selective fermentation, and separation of derivatives formed with optically active reagents—are still, a century later, the only general methods available to chemists. For this brilliant work, by virtue of which he takes rank as the founder of stereochemistry, Pasteur was awarded the Rumford Medal of the Royal Society in 1856. In France, however, his first presentation as a candidate for the *Académie des Sciences* was unsuccessful, and his election was delayed until 1862.

At Lille, Pasteur's attention was attracted to the application of fermentation in the production of alcohol from grain and beet-sugar. He was thus led on to study the souring of beer, and in 1864 he extended his inquiries to the cause of sourness, bitterness, and ropiness in the celebrated rosy and tawny wines of his native Arbois. He examined under his microscope the micro-organisms present in good beer, and compared them with those of sour beer. In both he found yeast cells, but in the sour beer he found also other cells which on investigation proved to be responsible for the unpleasant taste. He showed that the growth of these harmful organisms could be inhibited by gentle heat, and thus introduced the process which we now call pasteurization. The application of pasteurization to milk has since become, as everyone knows, one of the principal methods of preventing the spread of tuberculosis. Pasteur also studied the process of vinegar-making and was able to suggest valuable improvements in this important industry.

Among the far-reaching results of Pasteur's

biochemical work was his conclusive demonstration of the falsity of the theory of spontaneous generation, at that time still widely accepted. Somewhat surprisingly, Pasteur found his conclusions challenged, but he soon enlisted able supporters in Joseph Lister—who quickly perceived the great significance of Pasteur's work in his own field—and John Tyndall, the physicist. The Royal Society reaffirmed its confidence in him by electing him to membership in 1869 and awarding him the Copley Medal in 1874. The opposition eventually proved of value, however, for it led Pasteur to make new experiments, which so fully supported his earlier conclusions that they eventually proved unassailable.

From the study of micro-organisms which bring about the fermentation of wines and beers, Pasteur was led to the discovery of the bacterial causes of disease. This phase of his work began with his election to the *Académie de Médecine* in 1873. His candidature was opposed, and was approved by a majority of only one. Nevertheless, within ten years his work had introduced entirely new conceptions of the origin of certain diseases. In the years 1877–81 he made swift progress in this field. Much of his earlier experimental work (1865–8) was done with silkworms, for at that time the silk industry was in serious difficulties owing to the incidence of diseases which were destroying tens of thousands of the grubs. His work not only established his general theories but proved of immense practical value to the silk industry. From the diseases of silkworms he went on to the study of infectious diseases in general, and characterized many of the more important pathogenic bacteria. He discovered, too, the principles of inoculation by the use of attenuated strains of bacteria, thus placing the earlier work of Jenner on a logical basis. In this field his work on rabies, anthrax, and chicken cholera is particularly noteworthy. In 1881 he accepted a challenge to stage a public demonstration of his methods of immunizing cattle against anthrax. The demonstration, carried out before a large gathering of distinguished men, was wholly successful and did much to establish general acceptance of his views. It is impossible to compute the number of lives that have been saved by the general introduction of the principles of vaccination into medicine.

Not until 1887, however, was the controversy which had raged about Pasteur's work completely stilled. In the previous year the British Government had appointed a commission—which included James Paget, Joseph Lister, and Henry Roscoe—to study Pasteur's method for the treatment of rabies. In July 1887 they published a report substantiating every claim that Pasteur had made. A few days later he deposited a copy of the report at the offices of the *Académie des Sciences* in Paris. Of this event he said: 'During the long course of my scientific career I have never experienced a happiness equal to that which I felt on reading this report.'

Pasteur's work is notable for the breadth of understanding which he displayed in the study of widely differing phenomena. Any one of his great discoveries—in stereochemistry, in fermentation, or in bacteriology—would have assured him undying fame. Yet the validity of some of his work was questioned in France, and he was faced at times with violent opposition. That one man should have made so many brilliant discoveries, and despite bitter criticism, is truly amazing. His success, as already remarked, may be attributed in part to his pioneer use of chemical methods in the study of biological problems. He was entering an almost untouched field, and with so powerful a tool was able to reap a great and immediate harvest. His successors in the field of biological chemistry have made equally great discoveries, but few individual efforts have been comparable with his. 'Let us all work,' wrote Pasteur; 'that only is enjoyable.'

In 1887 subscriptions were invited for the building of a research institute in which Pasteur could worthily continue his work. There was an immediate response from all over the world, and on 14th November, 1888, the Pasteur Institute was opened in Paris. Today this building and other Pasteur Institutes in many different parts of the world form a fitting memorial to him and his achievements.

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The age of the earth

ARTHUR HOLMES

The history of the many efforts that have been made to discover the age of the earth's crust reveals a remarkable alternation of contrasted estimates—long and short. Kelvin applied a corrective by insisting that the time that has elapsed since the earth consolidated sets an upper limit to the duration of geological history. Professor Holmes here describes the latest refinement in methods based upon the detailed study of radioactive minerals, and concludes that from the data at present available the most probable estimate is 3,350 million years.

Long before it became a scientific aspiration to estimate the age of the earth's crust, many elaborate systems of world chronology had been devised by the sages of antiquity. The most remarkable of these occult time-scales is that of the ancient Hindus, who calculated that it is now (A.D. 1947) 1,972,949,048 years since the earth came into existence. By a curious coincidence this characteristically precise assessment is of the same order as the 2,000 million years which has recently been the most widely favoured estimate for the age of the expanding universe.

If geological concepts had developed in a community endowed in advance with so generous a concept of the past, much confusion and bitter controversy might have been avoided. But in western Europe the age of the earth had long been identified—to within a few days—with the few thousand years of mankind's history as recorded in the narratives of the Old Testament. On the interpretation of Archbishop Ussher (1581–1656), the creation of the world took place in the year 4004 B.C., and pioneer geologists whose observations suggested that the Mosaic traditions might not be scientifically reliable were branded as dangerous heretics.

A mild though significant instance of the prejudicial influence of this cramping limitation of time is afforded by a remark made by the renowned astronomer Edmund Halley (1656–1742) in the course of a communication to the Royal Society on a *Proposal . . . to discover the Age of the World* (1715). Halley realized that the sea had become salt because of the accumulation of saline material contributed by inflowing rivers, and he suggested that the total amount of salt in the sea might therefore provide a measure of the age of the oceans. At that time the necessary data for making the calculation were not available, and Halley lamented that the ancient Greek and Latin authors had not 'delivered down to us the degree of the saltness of the sea, as it was about 2,000 years ago; for then,' he continued, 'it can not be doubted but that the difference between what is now found and what was then, would become very sensible.' Now obviously, if Halley had been thinking in millions of years instead of thousands, he must have realized that the increase of salinity since Greek or Roman times would be quite undetectable. Nevertheless, he did not fail to express his suspicion that 'the world may be found much older than many have hitherto imagined.' It was James Hutton (1726–97) (see page 109) who first clearly grasped the full significance and immensity of geological time.

Today we know that the earth's history has included a succession of at least ten major cycles, each involving (1) thick accumulations of sediments and volcanic rocks in a subsiding belt of the crust; (2) intense compression of the belt, resulting in folding and crumpling, and accompanied by metamorphism of the deeper rocks and the formation of great masses of granite; and (3) general uplift of the belt and the wearing away of its exposed portions by denudation. The effects of such changes are illustrated in figures 1 and 2. The Alps and the Himalayas are examples of mountain systems now in stage (3) of the latest of these mountain-building or 'orogenic' cycles, as they are called. The rocks of Cornwall and Devon are relics of the immediately preceding cycle, and those of the Lake District and most of Scotland and Wales and Norway belong to the next earlier one. From beneath the North-West Highlands of Scotland and the mountains of Scandinavia the rocks of even older cycles appear (figure 3). So, going farther afield than Hutton did, we can travel back through a long series of these contorted records of earth history. But however far we penetrate into the past we still, like Hutton, find 'no vestige of a beginning.' How this comes about can readily be grasped by reference to figure 6 (page 104). In every continent the oldest known rocks are

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found to be metamorphosed sedimentary types which gradually merge into completely reorganized rocks such as granite. The granite, however, instead of being older than the rocks in which it is emplaced, is actually younger, since it crystallized in its present form during or after the folding of the pile of sediments with which it is associated. Everywhere the oldest visible rocks pass sideways or downwards into granitic foundations that are younger than themselves. But since the oldest visible rocks are of sedimentary origin, they must have been derived from rocks of still greater age: from rocks now represented by pebbles and sand grains, and of which no other recognizable trace has so far been found.

Hutton's experience covered only a small proportion of the earth's history now known to us, and he was therefore recording no more than the sober truth when he declared he could find 'no vestige of a beginning.' In the absence of guiding data, he made no attempt to estimate the rates of geological processes or the periods represented by their products. Many of his successors, however, exhilarated by their newly-found freedom, became unduly reckless in their extravagant demands for time. In 1859, for example, Darwin estimated from the supposed rate of chalk erosion in Kent that the time required for the denudation of the Weald and the recession of the bordering escarpments of the North and South Downs to their present positions was probably about 300 million years. We now know that this estimate was at least five times too long; but Jukes, commenting on it at the time, thought it quite as likely that the period required might have been a hundred times as long. Evidently 30,000 million years was not considered absurdly excessive for a small fraction of geological time.

Kelvin, one of the greatest pioneers of geophysics, then entered the field with a dramatic counterblast against the current assumptions of unlimited time. He argued that since there is a heat-flow through the earth's crust, measurable in terms of the downward increase of temperature and the thermal conductivities of rocks, the earth could be regarded as a cooling globe which must therefore have been progressively hotter in the past. Beyond the dim horizon of the oldest rocks he envisaged a 'beginning' corresponding to the time when the earth was molten and newly born from the sun. In 1862 he set himself the problem of calculating the time that had elapsed since the earth's consolidation. Because of uncertainty in many of the data, he allowed wide limits to his solution, concluding that the observed temperature gradients would have been notably lower than they are if the crust had solidified more than 400 million years ago, and notably higher than if solidification had been completed less than 20 million years ago.

Kelvin's challenge initiated one of the many scientific controversies that enlivened Victorian times. Despite many protests, however, he finally narrowed his limits to 20 and 40 million years (1897). Archibald Geikie pointed out in 1899 that the testimony of the rocks clearly denied Kelvin's thermodynamic inference that geological activities must have been more vigorous in the past than they are today, and that the known sequence of sedimentary strata could not have accumulated within the limits set by Kelvin's solution of the problem. Moreover, James Geikie (1900) showed very convincingly that the crustal compression set up by even 100 million years of cooling would be confined to an outer shell far too thin to accommodate the immense thicknesses of folded rocks involved in the Alps and other great mountain ranges. Evidently some factor—if not more than one—had been overlooked, though few physicists were then willing to admit the possibility of any fundamental mistake. Perry, however, had already heartened the geologists by pointing out that it was allowable to assume a relatively high thermal conductivity for the deep interior of the globe, in which case the cooling of the crust would have been correspondingly slowed down. He saw no reason for denying the geologists anything up to 4,000 million years, an estimate of an order that has been confirmed by Wasiutynski in the course of a recent discussion of the earth's thermal history (1946).

Nevertheless, Kelvin's great authority compelled a compromise, and at the turn of the century geologists who claimed more than about 100 million years were thought to be unduly rash. A few, indeed, reluctantly satisfied themselves with a more meagre allowance of time, but the majority steadfastly refused to accept Kelvin's results as final. The real flaw in Kelvin's assumptions was disclosed shortly after the discovery of radioactivity, when Strutt (Lord Rayleigh) detected the presence of radium in common rocks from all parts of the world. With the demonstration that the crustal rocks contain radioactive elements and are therefore endowed with an unfailing source of heat, it became obvious that the earth is not living merely on its ancestral capital of internal heat, as Kelvin had confidently believed, but that it has an independent and regular source of income

URE 1 (*above*) – *Upper Old Red Sandstone strata (age ut 280 million years) resting unconformably on vertical urian rocks (about 320 million years) at Siccar Point, Cock-spath, Berwickshire.*

(Photograph by H.M. Geological Survey, Crown copyright reserved)

FIGURE 2 (*below*) – *Carboniferous Limestone (about 240 million years) resting unconformably on inclined Silurian slates (about 320 million years) at Arco Wood Quarry, four miles north of Settle, Yorkshire.* *(Photograph by Professor S. H. Reynolds)*

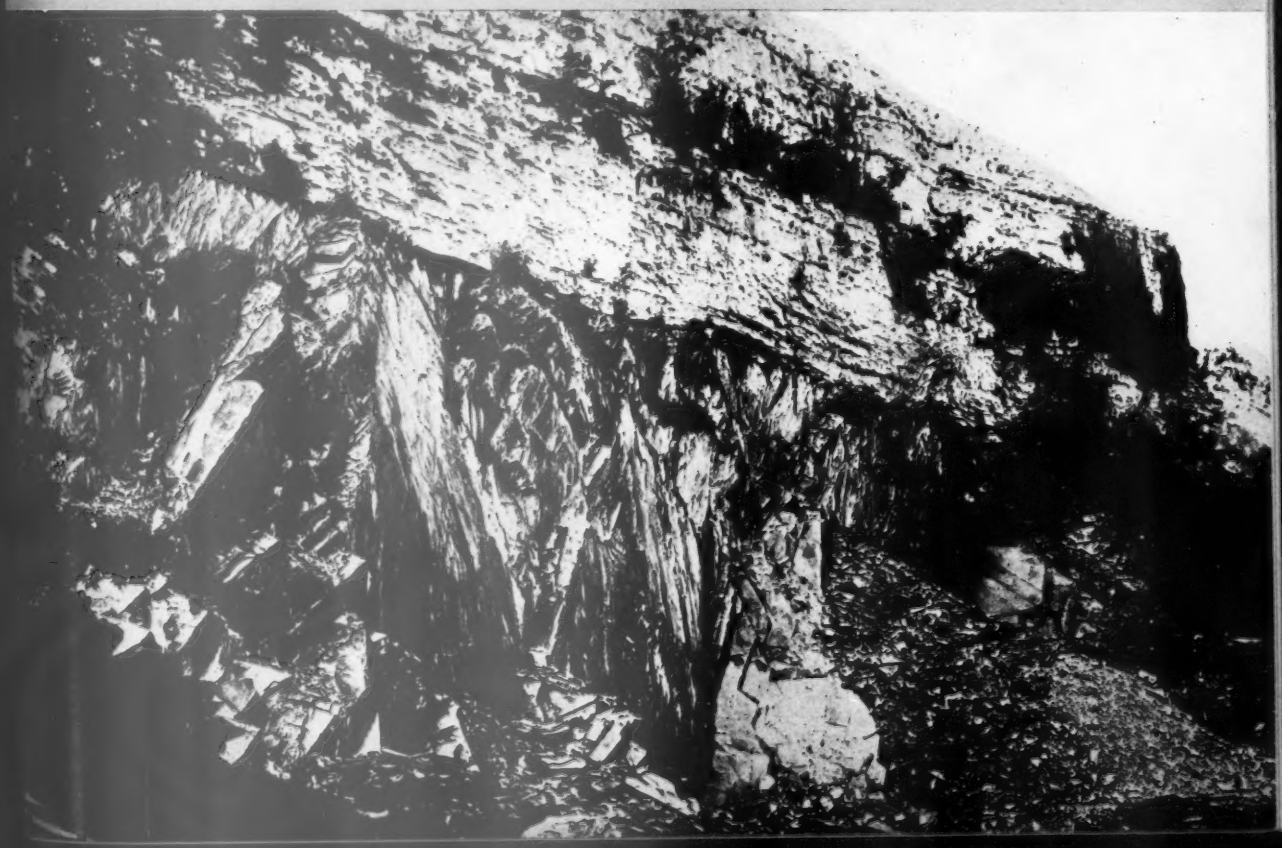

FIGURE 3 – *Wave-swept surface of folded rocks, metamorphosed and partly granitized, representing the worn-down roots of a Pre-Cambrian orogenic belt (about 1,050–1,100 million years), at Borgå, south coast of Finland.*
(Photograph by Professor C. E. Wegmann)

FIGURE 4 – *Pleochroic halo in mica from Murray Bay, Quebec, showing well-developed rings due to α-particles shot out from a uranium-bearing crystal at the centre.*
(Photomicrograph by Professor G. H. Henderson)

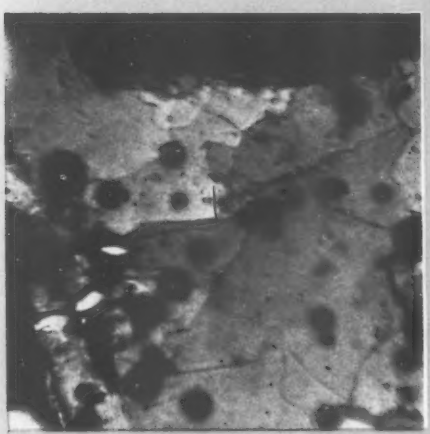

FIGURE 5 – *Pleochroic haloes in a flake of brown mica from a Pre-Cambrian granite (about 1,030 million years) from Ontario, Canada.*
(Photomicrograph by Professor G. H. Henderson)

of its own. The abundance of the radioactive elements in the crustal rocks is such that the net loss of heat is extremely low; age estimates based on the rate of cooling are therefore greatly increased. If, for example, nine-tenths of the heat-flow through the crustal rocks is of radioactive origin—and something of this order is consistent with the available evidence—then the alleged 20–40 million years of cooling has to be multiplied by 100 or more.

By this time the minds of the older geologists were no longer attuned to thinking in terms of thousands of millions of years, and few of them were prepared to take advantage of the new discoveries. This reluctance was largely due to the fact that in 1898 Joly had resuscitated Halley's suggestion for determining the age of the oceans. Adopting the simple hypothesis that—on an average—the annual amount of dissolved sodium removed by rivers from the land has remained constant throughout geological time, he found that about 80–90 million years would be required to furnish the total amount of sodium now present in the oceans. This estimate he shortly afterwards increased to 100 million years. A few years later Sollas concluded that the probable limits were 80–150 million years. In 1910 Becker, thinking it likely that the annual increments had been progressively greater in the past, reduced the probable age to about 60 or 65 million years. Indeed, Becker was sufficiently convinced of the validity of these 'short' estimates to declare that 'radioactive minerals cannot have the great ages which have been attributed to them.'

It is therefore of some importance to consider the present status of the sodium method. It appears from recent discussion of the relevant geochemical statistics (see especially Conway, 1942 and 1943) that practically all the 'chloridized' sodium in river water represents oceanic salt, either blown inland and washed down by rain, or derived from saline deposits or the pore-spaces of sediments. Allowing for such 'second-hand' salt, the present annual addition of new sodium is about 6×10^7 tons. Even this figure may be misleadingly high, because, as Lane has pointed out, analyses are rarely made of the water from rivers when they are in flood, i.e. when the content of dissolved material is at its minimum. The total accumulation of marine sodium in ocean water and sediments is estimated at about 15×10^{15} tons, less the amount initially present, which may or may not be negligible. Thus, on the assumption of past uniformity, all we can conclude is that the apparent age of the oceans is something of the order of 250 million years. But past uniformity can by no means be assumed. Present rates of weathering and erosion are far too abnormally high to be representative of the geological past. Mountain ranges and land areas in general are now much more elevated and extensive than has usually been the case. Rivers and ground waters are therefore unusually active, and, moreover, many of them drain regions that are thickly strewn with easily weathered glacial deposits. Finally, human activities of all kinds—agricultural, engineering, and chemical—have still further speeded up the rates of weathering and erosion over widespread areas.

Only a crude attempt can be made to assess the effects of these considerations on the 'apparent' age of the oceans. Figure 6 illustrates the observed fact that the continents are made up of belts of rocks that have suffered intense compression, folding, and thickening. If we imagine—going backwards in time—all the folds to be straightened out, then the crustal layer responsible for the continents becomes increasingly attenuated and extensive; so much so that in the early days of geological history this continental layer would be spread out so thin that its surface would be below sea level, and the only lands would be a few volcanic islands. Since then the continental lands exposed to erosion have, on the whole and despite wide fluctuations, progressively increased in height and area. According to this conception, the quantitative effects of denudation must have gradually increased from almost zero at the beginning to the all-time maximum of the present epoch. Roughly we may reckon that the time-average rate was probably between $\frac{1}{2}$ and $\frac{1}{3}$ of the present rate in respect (separately) of area, height, and relief. That is, it was probably between $(\frac{1}{2})^3$ and $(\frac{1}{3})^3$ of the present rate. The age of the oceans may therefore be anything between 8 and 27 times the apparent age of 250 million years, which itself may be a minimum. Obviously the hour-glass of sodium accumulation is a hopelessly variable timekeeper. The most that can be said is that its present reading is not inconsistent with an oceanic age of a few thousands of millions of years.

What we need for the accurate measurement of such immense periods is a natural process that has operated throughout geological time and has produced measurable results at a known rate, of which the law of variation with time is also known. The decay of the radioactive elements is the only known process that fulfils these stringent conditions. The radioactive methods depend on the

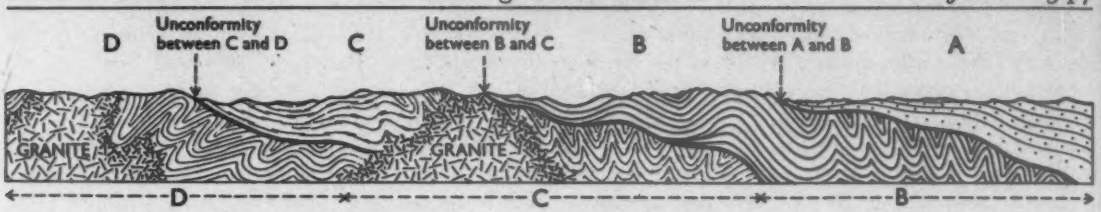

FIGURE 6 – *Diagrammatic section to illustrate the rocks and structures of a series of orogenic belts (A, B, C, and D) such as make up the continental crust of the earth. (Vertical scale greatly exaggerated.)*

transformation of uranium and thorium into helium and lead, and on the accumulation of these stable end-products in minerals and rocks that contain the parental elements. Helium, being a gas, is liable to escape, but the lead is much more likely to be retained and so to serve as an index of age. Provided a radioactive mineral such as pitchblende or uraninite has remained unaltered by weathering or other changes, then the amount of radiogenic lead now found within it is a function of (*a*) the amounts of uranium and/or thorium now present and (*b*) of the time elapsed since the mineral first crystallized. Fortunately it is possible to discriminate between the radiogenic lead and any ordinary lead that may have been present as an initial impurity in the mineral. The parental uranium contains two chemically inseparable isotopes, U_I (or U^{238}) and AcU (or U^{235}), in atomic proportions having the present value $AcU/U_I = 1/139$. Since AcU decays much more rapidly than U_I, this ratio was progressively higher in the past. The material results of the atomic transformations can be summarized as follows:

$$U^{238} \rightarrow Pb^{206} + 8He$$
$$U^{235} \rightarrow Pb^{207} + 7He$$
$$Th^{232} \rightarrow Pb^{208} + 6He$$

It will be noticed that in each case a specific isotope of lead is generated. Ordinary lead is a mixture of the same three isotopes, together with a fourth, Pb^{204}, which is not known to be an end-product of radioactive decay. Thus, if the lead separated from a radioactive mineral is isotopically analysed (e.g. by means of the mass-spectrograph) and found to contain Pb^{204}, the proportion of the latter provides an index of the amount of ordinary lead that was initially present.

The present rates of production of radiogenic lead are known with a remarkable degree of accuracy, but the question naturally arises: can we be reasonably sure—apart, of course, from the inevitable slowing down due to the wearing out of the parents—that these rates have remained constant throughout geological time? In other words, can we be sure that the physical constants concerned have not varied with time? Fortunately, pleochroic haloes provide us with an unambiguous affirmative. Certain granites contain flakes of brown mica which, under the microscope, can be seen to be sprinkled with dark circular spots (figure 5, page 102). These are known as pleochroic haloes, and some of them, when highly magnified, reveal a beautifully developed pattern of concentric rings (figure 4, page 102). A minute radioactive crystal lies at the centre of each halo, and the darkening of the surrounding mica is produced by the helium atoms (α-particles) that are shot out in all directions. The radius of each ring corresponds to the range of the α-particles from one particular radioactive element. Careful measurements by G. H. Henderson (1943) show that the rings in Pre-Cambrian haloes over 1,000 million years old are just as sharply defined as those in younger rocks, and that the corresponding radii and ranges are identical. Since the range, in turn, depends on the rate of disintegration of the radioactive element concerned, it follows that the radioactive constants have not varied appreciably for at least 1,000 million years.

At any given time the rate of production of a particular lead isotope depends only on the disintegration constant and amount of the parental element then present. Thus the age of a mineral t_m can be readily calculated from each of the three ratios Pb^{206}/U, Pb^{207}/U, and Pb^{208}/Th, where these symbols here represent the percentages of the parent elements and of the isotopes of radiogenic lead now present in the mineral under investigation. The respective equations for t_m are (Keevil, 1939):

$$t_m = 15{\cdot}15 \times 10^9 \log_{10} (1 + 1{\cdot}158\ Pb^{206}/U) \text{ years}$$
$$t_m = 2{\cdot}37 \times 10^9 \log_{10} (1 + 159{\cdot}6\ Pb^{207}/U) \text{ years}$$
$$t_m = 46{\cdot}20 \times 10^9 \log_{10} (1 + 1{\cdot}116\ Pb^{208}/Th) \text{ years}$$

A fourth value for t_m can be found from the ratio Pb^{207}/Pb^{206}.

If a mineral has remained unaltered, then all

four values for t_m should be in close agreement. In practice this rarely happens, because, even in the freshest-looking mineral, migrations of the critical elements are likely to have occurred. Fortunately, even if the three values for t_m based on Pb^{206}/U, Pb^{207}/U, and Pb^{208}/Th are widely different the relations between them provide criteria for assessing the true age (figure 7). If there has been leakage of radon (the gaseous member of the U1 family), then the mineral is necessarily deficient in Pb^{206}; in this case the most probable age is given by Pb^{207}/U (see Wickman, 1942, and Holmes [5], 1947*b*). If there has been loss of Pb or U or both, or gain of either or both, then the true age, or a close approximation to it, is given by Pb^{207}/Pb^{206}.

Although a great many radioactive minerals have been chemically analysed, relatively few isotopic analyses of the lead have been made as yet. Professor A. O. Nier, following up Aston's pioneer work, has been the most active and successful worker in this field. As an example of a fully investigated mineral, samarskite from Spinelli Quarry, Portland, Connecticut, may be taken. The mineral occurs in a pegmatite dating from about the end of the Devonian period.

	Pb	*U*	*Th*	*'Apparent Age' from total Pb (million years)*
Percentages (R. C. Wells)	0·314	6·91	3·05	295

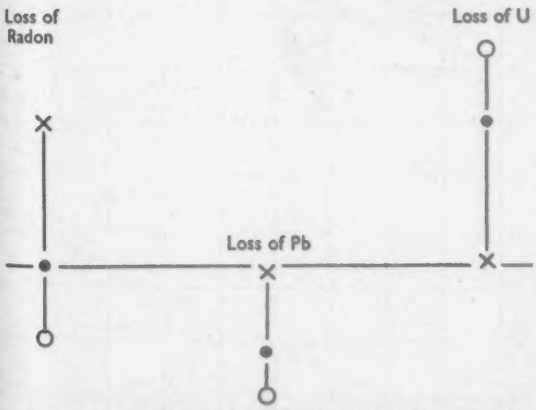

FIGURE 7 – *Diagram to illustrate the effects of various types of alteration on the apparent ages of a radioactive mineral calculated from Pb^{206}/U (open circles); Pb^{207}/U (solid circles); and Pb^{207}/Pb^{206} (crosses). The horizontal line represents the true age of the mineral, points above and below being respectively too high and too low.*

Isotopic Proportions (Nier *et aliter*)	Pb^{204}	Pb^{206}	Pb^{207}	Pb^{208}
In total lead ..	0·167	100·00	7·60	21·30
In original lead	0·167	3·04	2·61	6·35
In radiogenic lead	—	96·96	4·99	14·95
Percentages ..	—	0·236	0·012	0·036
Age Ratios	$\frac{Pb^{207}}{Pb^{206}}$	$\frac{Pb^{208}}{U}$	$\frac{Pb^{207}}{U}$	$\frac{Pb^{208}}{Th}$
	0·0515	0·03415	0·00174	0·0118
Values of t_m (million years)	256	255	254	266

Here the agreement is unusually good, and it can be concluded that about 255 million years have elapsed since the close of the Devonian period.

In the following table some of the better-established dates are listed:

Mineral	*Locality*	*Geological Age*	*Probable Age (millions of years)*
Pitchblende	Colorado	Beginning of Tertiary	58
Pitchblende	Bohemia	Late Carboniferous	215
Samarskite	Connecticut	End of Devonian	255
Cyrtolite	New York	End of Ordovician	350
Kolm	Sweden	Upper Cambrian	440
Pitchblende	Katanga, Belgian Congo	Pre-Cambrian	580
Uraninite	Morogoro, Tanganyika	Pre-Cambrian	590
Uraninite	Besner, Ontario	Pre-Cambrian	760
Bröggerite	Moss, S. Norway	Pre-Cambrian	860
Uraninite	Wilberforce, Ontario	Pre-Cambrian	1,035
Cleveite	Aust Agder, S. Norway	Pre-Cambrian	1,075
Pitchblende	Great Bear Lake, Canada	Pre-Cambrian	1,330
Uraninite	N.E. Karelia, U.S.S.R.	Pre-Cambrian	1,765
Uraninite	Huron Claim, Manitoba	Pre-Cambrian	1,985

The uraninite from Manitoba is the oldest mineral so far investigated, and its great age is roughly confirmed by analyses of another uraninite and of two monazites, all from the same pegmatite. The 'apparent ages' of these, calculated from the total lead, are 1,950, 1,955, and 1,990 million years respectively. The pegmatite represents the closing phase of the plutonic activity of a typically Archaean orogenic belt. It followed a long series of granitic and other plutonic rocks, which in turn are emplaced within a thick sequence of metamorphosed volcanic and sedimentary rocks. The

latter include still recognizable conglomerates, containing pebbles of pre-existing granites and quartzites, which must therefore be well over 2,000 million years old. Since the earth must be older still, this figure can be regarded as a conservative minimum for its age.

To find a maximum for the age of the earth we may assume that when the earth began it was free from the lead isotope Pb^{207}, and that all the Pb^{207} now present in the common granitic rocks of the continental crust has since been generated from U^{235}. Granitic rocks contain on an average about 20 parts per million of lead and 3·5 parts per million of U. The isotopic constitution of granitic lead has not yet been determined directly, but isotopic analyses of several samples of lead from galena and other lead ores of Tertiary age have been made by Nier and his co-workers. These ores represent concentrations of the granitic lead of some 25 million years ago, which is near enough to the present for our purpose. The average isotopic abundances of Tertiary lead are:

Pb^{204}	Pb^{206}	Pb^{207}	Pb^{208}	*Total (parts per million)*
1	18·54	15·55	38·28	73·37

corresponding in parts per million very nearly to

0·27	5·1	4·2	10·4	20

in the average granitic rocks of today. Inserting the value $Pb^{207}/U = 4·2/3·5$ in the appropriate formula given on page 104, the time required for the generation of all the Pb^{207} is found to be 5,400 million years. The age of the earth is therefore somewhere between 2,000 and 5,400 million years.

Can we arrive at a closer estimate? I think we can, again by making use of the invaluable data provided by Nier (see Holmes, 1946). Nier and his co-workers have determined the relative abundances of the isotopes in twenty-five samples of lead from common lead minerals of various geological ages. The results, graphically summarized in figure 8, reveal internal relationships between Pb^{206}, Pb^{207}, and Pb^{208} that are almost exactly what they would be if the primeval lead originally present in the material of the outer earth had been slowly modified by additions of radiogenic lead. A few of the samples have a more or less abnormal constitution, but, apart from these, the evidence is clear that ore-lead represents a concentration of the lead that was dispersed through the crustal rocks of the region concerned at the time when the ore deposition took place.

The problem, then, is this: knowing the isotopic constitution of rock-lead as it was at various periods ranging from 1,330 million years ago (the Great Bear Lake sample) to 25 million years ago (the Tertiary samples), to find the relative abundances of Pb^{206} and Pb^{207} in the earth's primeval lead and the time that has elapsed since that primeval lead began to be modified by radiogenic additions. This time (t_o) is the required age of the earth.

Using the following symbols, with t_m for the age of the lead ore:

	Pb^{204}	Pb^{206}	Pb^{207}	Pb^{208}
Ore-lead (≡ rock-lead) ..	1	a	b	c
Primeval lead	1	x	y	z
Radiogenic lead (from t_o to t_m)	—	$a-x$	$b-y$	$c-z$

we can write:

$$\frac{b-y}{a-x} = r = \frac{\text{No. of atoms of } Pb^{207} \text{ generated from } t_o \text{ to } t_m}{\text{No. of atoms of } Pb^{206} \text{ generated from } t_o \text{ to } t_m}$$

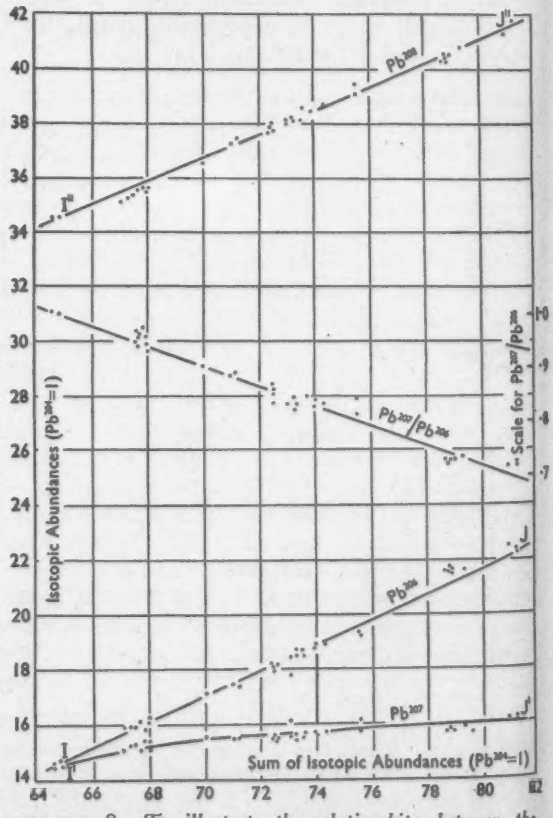

FIGURE 8 – *To illustrate the relationships between the abundances of Pb^{206}, Pb^{207}, and Pb^{208} (relative to Pb^{204} = 1) in lead samples prepared from common ores of lead.*

From the expression on the right r can be calculated (for each appropriate value of t_m) for various assigned values of t_o from 2,000 to 5,000 million years. For each assigned value of t_o we have:

$$ar - xr = b - y \quad \text{for a lead sample of age } t_m$$

and $$a'r' - xr' = b' - y \quad \text{for a lead sample of age } t_m'$$

whence $$x = \frac{b - b' + a'r' - ar}{r' - r}$$

and $$y = b + rx - ar$$

Taking samples 25 and 18 (figure 10, page 108), the following results are obtained:

No. 25 *Galena, Great Bear Lake (Pre-Cambrian)*	*No.* 18 *Galena, N. Carolina (Late Carboniferous)*
$a = 15{\cdot}93$	$a' = 18{\cdot}43$
$b = 15{\cdot}30$	$b' = 15{\cdot}61$
$t_m = 1{,}330$ million years	$t_m' = 220$ million years

When $t = 2{,}500$ million years:

$r = 0{\cdot}23305$ $\quad r' = 0{\cdot}16999$

$x = 14{\cdot}11$ $\quad y = 14{\cdot}87$

When $t = 3{,}000$ million years:

$r = 0{\cdot}30156$ $\quad r' = 0{\cdot}22794$

$x = 12{\cdot}40$ $\quad y = 14{\cdot}24$

When $t = 3{,}500$ million years:

$r = 0{\cdot}39685$ $\quad r' = 0{\cdot}30985$

$x = 10{\cdot}59$ $\quad y = 13{\cdot}18$

Plotting x against t, a curve AB is drawn; a point representing a solution for x and t_o lies somewhere on this curve or its continuation. Dealing in the same way with another pair of lead samples, such as No. 1 (galena, Peru, Tertiary, 25 million years) and No. 19 (galena, Ivigtut, Greenland, Late Pre-Cambrian, 600 million years), a curve CD is similarly constructed. Where the two curves intersect (at P in figure 10) $x = 1{\cdot}22$ and $t_o = 3{,}330$ million years.

In the same way we can plot y against t and construct two curves that intersect at $y = 13{\cdot}71$ and

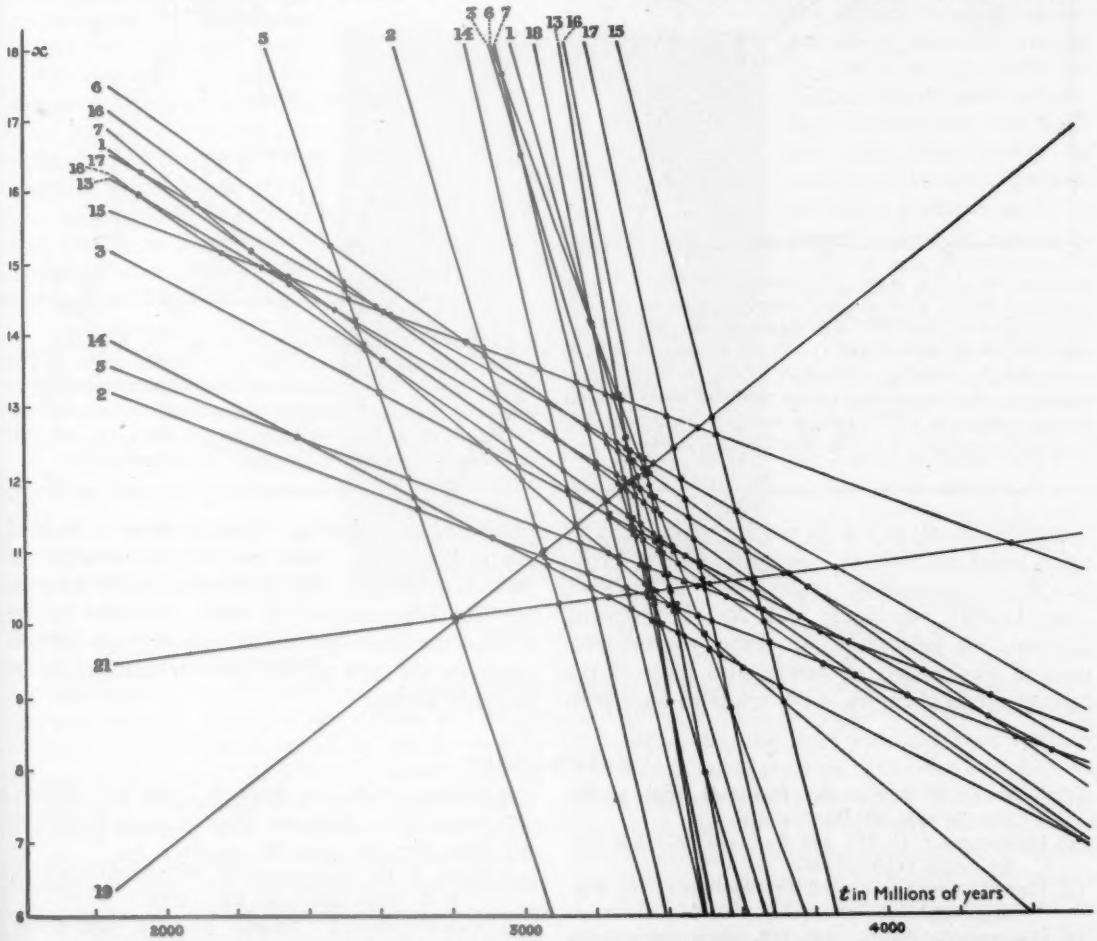

FIGURE 9 – *Each of the curves numbered on the left corresponds (like AB in Figure 9) to a pair of lead samples, of which No. 25 is one. The curves similarly based on No. 19 are numbered along the top. The numbers are those of Nier's lead samples as listed in Holmes (1946). Each intersection, marked by a dot, provides a solution for x and t_o.* (*See page* 108.)

t_o = 3,330 million years. The two pairs of curves, based on the data for two pairs of lead samples, thus yield a solution of the problem.

Obviously a great many solutions can be obtained by this method. Figure 9 illustrates the *xt* curves for pairs made up of No. 25 with each in turn of fourteen younger lead samples, and for pairs made up of No. 19 with 11 younger lead samples. The intersections provide over two hundred solutions, with a marked concentration at about 3,300–3,400 million years.

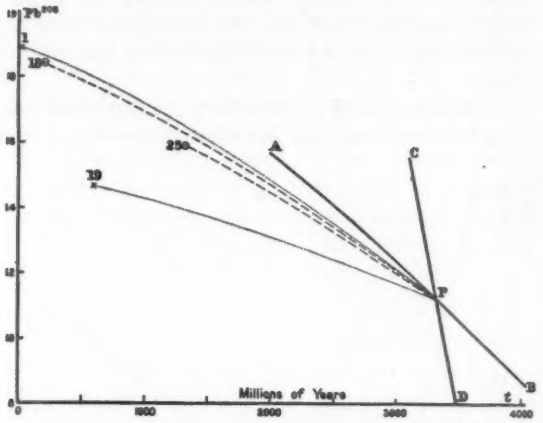

FIGURE 10 – *The curve AB indicates the variation of x (primeval Pb^{206}) with assigned values of t for the pair of lead samples 25 and 18. CD represents the corresponding variation for the pair 19 and 1. The intersection at P gives a solution for x and t_o. The lines joining P to the points representing the lead samples (a and t_m in each case) show the gradual increase of Pb^{206}, relative to Pb^{204} = 1, from x to a during the period t_o–t_m.* (*See page* 107.)

obtained for t_o, *x*, and *y*. The average value of t_o is 3,290 million years, but a histogram of the t_o values (figure 11) reveals a well-marked mode at 3,350 million years, which is found to be somewhat better than the average value when tested by a least-squares method (Holmes [5], 1947*a*). The dotted frequencies on the left of figure 11 illustrate the lopsided effect of including results involving the data for two lead samples of

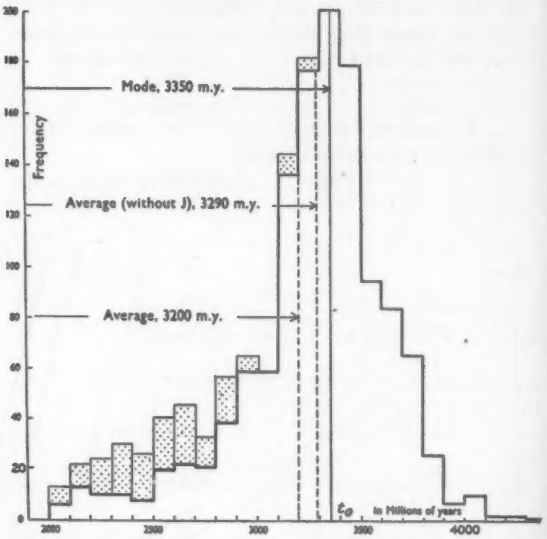

FIGURE 11 – *Histogram representing the frequency distribution of 1,419 solutions for t_o to within ranges of 100 million years. The frequencies of 162 solutions involving the abnormal Joplin lead samples (Nos. 9 and 11) are indicated by the dotted areas. The histogram for the 1,257 solutions without the aberrant Joplin results is outlined boldly.*

Additional sets of curves can be constructed for pairs based on No. 23 (cerussite, Broken Hill, N.S.W., 1,200 million years); No. 22 (galena, same locality and age); and No. 21 (galena, Quebec, 800 million years). From the intersections of all five sets of curves, and those of the corresponding *yt* curves, 1,257 solutions have been obtained. The average value of t_o is 3,290 million years, but a histogram of the t_o values (figure 11) reveals a well-marked mode at 3,350 million years, which is found to be somewhat better than the average value when tested by a least-squares method (Holmes [5], 1947*a*). The dotted frequencies on the left of figure 11 illustrate the lopsided effect of including results involving the data for two lead samples of abnormal constitution. Certain other abnormal leads, however, would give a concentration of results on the right. The fact that the modal solution remains independent of these aberrant results favours the hope that an estimate of 3,350 million years for the age of the earth is unlikely to be seriously wrong.

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James Hutton, the founder of modern geology: 1726–97

M. MacGREGOR

The second half of the eighteenth century was a period of intense intellectual activity in Scotland. Among the greatest of the many distinguished figures of the time was James Hutton, whose geological researches and writings established the fundamental principles of modern geology. He was the first to recognize and describe the processes which determine the past and present configuration of the earth, drawing his evidence not from the academic speculations in which his contemporaries indulged but from the rocks themselves.

The 26th of March, 1947, saw the hundred and fiftieth anniversary of the death of James Hutton, who has been so often acclaimed as the founder of modern geology. The memory of this great Scotsman is still green, not only in the little northern kingdom where he was born and where he laboured, but throughout the scientific world. To the generality of people he is perhaps best known as the author of the often-quoted saying that in the operations of nature 'we find no vestige of a beginning—no prospect of an end,' words that were startling and indeed iconoclastic when they were written in 1785. At this distance of time it is difficult for us to measure the debt we owe to Hutton's genius or to realize how profoundly the principles he established came to revolutionize the whole trend of scientific speculation. To appreciate the fundamental importance of his work and its far-reaching repercussions, it must be set against the background of the time in which he wrote; and to return to the latter half of the eighteenth century is to enter a new and strange geological world, differing from our own in its concepts and scope. It was a world given over to dogmatisms and preconceived theories, to fantastic explanations invoked to account for natural phenomena, to barren speculations that sought to bring the facts of nature into conformity with the traditional Mosaic chronology. A universal ocean in which the solid rocks now forming most of the dry land had been deposited as chemical precipitates, a succession of sudden, catastrophic events such as the violent, tumultuous upheaval of mountain ranges, and above all, the limitation of the time available for geological changes to a few thousand years, were amongst the commonly accepted tenets of the time. Under these conditions progress in geological research was narrowed and obstructed, and it was Hutton who, by uprooting and sweeping away all such sterile conceptions, placed geology on a solid and secure foundation. To free geology of its accumulated traditions, false analogies, and misconceptions was a Herculean task, but it was a task which he carried out quietly and courageously. To study and understand the processes at work on the surface of the earth, to frame a system of philosophy which would explain them, and to show from the records of the solid rocks themselves that the same processes have operated throughout immeasurable time—this was the work to which he devoted long years of patient observation and reflection. His *Theory of the Earth* is unquestionably one of the great classics of science, by virtue of its wide sweep, its originality, and its irresistible logic. There were no assumptions, no unverified propositions in his doctrine, no principles allowed that were not first submitted to the test of observation. He uses the word 'observable'

Reproduction of medallion portrait of James Hutton, M.D.

in the sub-title of his famous memoir of 1785, describing it as 'An Investigation of the Laws observable in the Composition, Dissolution, and Restoration, of Land upon the Globe.' Nature herself was to him the ultimate court of appeal. In one memorable passage he wrote: 'It is only in knowing the succession of things, that natural appearances can be explained; and it is only from the examination of those appearances, that any certain knowledge of this operation is to be obtained. But how shall we acquire the knowledge of a system calculated for millions, not of years only, nor of the ages of man, but of the races of men and the successions of empires? There is no question here with regard to the memory of man, or any human record, which continues the memory of man from age to age; we must read the transactions of time past, in the present state of natural bodies; and, for the reading of this character, we have nothing but the laws of nature, established in the science of man by his inductive reasoning.'

HUTTON'S LIFE AND TIMES

Hutton's life-story can be simply told. He was born at Edinburgh on 3rd June, 1726, the son of a merchant who had for a time held the office of City Treasurer. He studied medicine for three years at Edinburgh University and for two years in Paris before taking his doctor's degree at Leyden in 1749. On his return home he decided to abandon medicine for farming, and with this end in view spent two years (1752–4) in Norfolk learning agricultural methods. These years in Norfolk were important and formative ones, because it was at this period that his early passion for scientific research turned definitely in the direction of geology. In 1754 he settled down in Berwickshire, where he devoted himself to the cultivation and improvement of the little estate which had been bequeathed to him by his father. He continued his geological investigations, however, making many excursions to different parts of the country and amassing a wealth of observations and records which he was later to put to such splendid use. About the year 1768 he determined to let his estate and to retire to Edinburgh, and it was in Edinburgh that the last twenty-nine years of his life were passed. It was retirement but in no sense leisure, for Hutton was a man of unwearied industry. He had many interests—philosophy, chemistry (he was partner in a firm manufacturing ammonium chloride), meteorology, and agriculture. He published volumes on physics and metaphysics, including a three-volume treatise entitled *An Investigation of the Principles of Knowledge, and of the Progress of Reason from Sense to Science and Philosophy*. He made a number of important meteorological studies, and he left behind him in manuscript a lengthy treatise on agriculture (now in the possession of the Geological Society of Edinburgh). But his main passion and his main life-work lay in the study of the rocks, minerals, and soils of his native land.

In Edinburgh he had a number of intimate and scholarly friends who took a keen interest in his work, and with whom he could discuss the new ideas which filled his mind: Joseph Black (1726–99), the distinguished chemist, who has sometimes been termed the father of modern chemistry just as Hutton himself has been called the father of modern geology; John Clerk of Eldon, 'that country gentleman who, with pieces of cork on his own dining table, invented modern naval warfare'; John Playfair (1748–1819), who was in later years to write a biographical account of his friend and to produce that inspiring and lucid masterpiece *Illustrations of the Huttonian Theory of the Earth* (1802); and Sir James Hall of Dunglass (1762–1831), who has recorded that 'after three years of almost daily warfare with Dr Hutton on the subject of his theory, I began to view his fundamental principles with less and less repugnance,' and who was later to bring powerful experimental support to Hutton's view that crystalline rocks could under certain conditions be produced by the cooling of originally molten material. It was in 1785, some seventeen years after his return to Edinburgh, that Hutton read before the Royal Society of that city his memoir or essay entitled *Theory of the Earth*. This memoir, occupying ninety-six quarto pages, was published in the first volume of the Society's *Transactions*.[1] A violent and unworthy attack on his *Theory* by a Dublin mineralogist in 1793, an attack which not only travestied Hutton's views but accused him of atheism, made him decide to extend and revise his 1785 memoir and to prepare and publish a more elaborate and complete statement of his position. In this task, weakened by illness as he was (he was now 67 years old), he persevered resolutely, and the immortal *Theory of the Earth, with Proofs and Illustrations* appeared in two octavo volumes in 1795. A third and concluding volume was left in manuscript at Hutton's death in 1797. The only four chapters of this volume that are

[1] See 'The Royal Society of Edinburgh,' by Professor J. Kendall (ENDEAVOUR, V, 18, April 1946).

known to have been preserved were published in 1899 by the Geological Society of London.

THE HUTTONIAN THEORY

Hutton's *Theory of the Earth* was wide-ranging in its scope, and the many unique contributions it made to geological research are without parallel in the history of the science. It was little noticed during his lifetime, and, indeed, the long controversy between the Neptunists, as those who adhered to the doctrine of a universal and gradually subsiding ocean were called, and the Plutonists, as the followers of Hutton were termed, did not die away until the first quarter of the nineteenth century had gone. It may be useful to give here one illustration of the divergence of views between the two schools. Granite, according to the Neptunists, was a rock of aqueous origin, the first to be laid down as a precipitate from the hypothetical universal ocean. Hutton, on the other hand, taught that granite had once been molten and had been intruded among the rocks in which it is found; and he pointed triumphantly to the observations he had made in Glen Tilt to show that granite veins emanating from the parent body entered and penetrated the surrounding rocks. He was beyond doubt the founder of physical and dynamical geology. He was one of the earliest geologists to stress the denuding and erosive power of running water and the part it played in the formation of valleys; he was the first geologist to recognize that denudation and deposition were closely linked aspects of the same natural process; and he was the first to see clearly that a succession of slow and slight effects would lead inevitably, if given a sufficient length of time, to widespread physiographic changes. It was consummate genius that led him to take the next step in the development of his argument. He turned to the solid rocks to find in them evidence that the same processes of change as he saw in the world around him must have operated again and again in the remote past. As he himself phrased it: 'the present is the key to the past.' The scenery of today is but the latest phase in a long-continued and ever-changing succession of landscape panoramas. One land mass is worn down and the waste products provide the materials for a new one, and continent follows continent in the eternal revolution of nature. Such, in outline, was Hutton's theory, and to the process as he envisaged it he could see neither beginning nor end.

He was the first geologist to recognize the importance of unconformities as providing clear proofs of the cycle of destruction and reconstruction, citing the now classic examples in North Arran, in the Jed near Jedburgh, and at Siccar Point on the Berwickshire coast. He realized that there must exist in nature some powerful force or agency which could elevate the solid rocks formed on the sea floor into new lands, and so initiate a fresh cycle of erosion. This force he found in subterranean pressures, due essentially, he believed, to the effect of heat transmitted from the interior of the earth. It was only by the action of powerful upward pressures that, to quote his own words, 'every species of fracture, dislocation, and contortion . . . and every degree of departure from a horizontal to a vertical position' seen in the uplifted strata could be explained. When he speaks of rock masses as having 'undergone a double course of mineral changes and displacement,' adding that 'consequently the effect of subterranean heat or fusion must be more apparent, and the marks of their original formation more and more obliterated,' geologists will recognize the germ of the hypothesis of regional metamorphism.

In the sphere of igneous geology Hutton's researches were of profound significance. He was the first geologist to assert and to demonstrate the essential differences between molten material poured out at the surface (lavas) and molten material solidified at depth and under great pressures. He was the first to recognize the intrusive character of the granites, whinstones, and porphyries which he encountered in his travels, and of the dykes and veins emanating from the deeper-seated eruptive masses. He noted also the disturbances and the displacements they caused during their intrusion, and the metamorphic effects (to use a modern phrase) they produced upon the strata they cut.

As has been already said, Hutton's *Theory* received little attention during his lifetime. But there was no gainsaying its originality, its magnitude, and its grandeur of conception. It contains the first statement of many of the most fundamental principles of modern geology. 'The very obviousness and familiarity of his doctrine at the present time,' wrote Sir Archibald Geikie, 'when it has become the groundwork of modern geology, are apt to blind us to the genius of the man who first conceived it, and worked it into a harmonious and luminous whole.' Other workers had gone some way along the path that Hutton travelled; he alone reached the mountain tops from which he could see, as it were, the past history of the world unroll itself before him.

The chemistry of glass coloration

L. M. ANGUS-BUTTERWORTH

The making and working of coloured glasses are among the world's most ancient crafts. Although the empirical methods of the early workers produced results of great beauty, the study of the chemical processes involved has proved a fruitful field of research. The present extensive use of coloured glasses in scientific apparatus, signalling lamps, photographic filters, and other equipment demands a standardized product which is obtainable only as a result of knowledge of the chemical reactions involved and careful control of manufacturing processes.

INTRODUCTION

The history of coloured glasses is longer than that of the transparent white variety. For many centuries before clear glass appeared a large number of different coloured forms had been in common use.

Some of the glazed tiles covering the walls of early Egyptian chambers were magnificent examples of colouring, and polychromes as well as single colours were achieved. Pieces have been found of a vase, bearing the name of a king living in 5000 B.C., which was glazed in green inlaid with purple. Some of the most important colouring oxides employed in glassmaking today, including those of copper, manganese, and cobalt, were known to the Egyptians. Transparent glass did not appear in Egypt before about 660 B.C., when bottles and a few other objects were made of it.

From these early beginnings in Egypt the art of coloured glassmaking gradually spread. One of the first uses of coloured glass in England was in cathedral windows.

A common belief is that we do not know today how to reproduce the brilliant colours of the early cathedral windows. This was partly true up to about a hundred years ago, but today the glass technologist certainly knows far more about colours in glass than was ever known before.

A further point in connection with old cathedral glass is that when originally placed in position it was often very crude in colour and dark in shade. It has taken hundreds of years to become mellowed. Atmospheric attack on the outside of church windows has been the chief agent in creating the beautiful effects we know today. Throughout the centuries there has been steady abrasive action through the rain being driven against the glass by the wind, and chemical dissolving action through the slight acid content of much of our rain water, especially in towns. The combination of these two forces has gradually changed thick and comparatively dark glass to glass which in some cases is as thin as tissue paper and which now has light and vivid colours.

Even the impurities in the metals, which the early glassmakers could not control, gave variety to their products. It was of little consequence if the glass they supplied for church windows never had quite the same colour twice. Exciting surprises were possible—not as in the dull modern world, in which glassmaking materials and processes are so carefully and completely controlled that results are (or should be) well ascertained in advance.

It is convenient to consider the agents for the production of each of the common colours in glass in turn.

BLACK

Cobalt and nickel oxides are used together to give opaque black glasses. Nickel glasses are noted by Professor Woldemar Weyl as absorbing nearly the entire visible spectrum with the exception of the extreme red.

A small antique glass bowl of an almost jet-black colour shown to the author on one occasion by the late Sir William Boyd Dawkins had certainly a high concentration of manganese as the principal colouring agent, possibly with the addition of a little cobalt.

BLUE

Cobalt and copper serve as the usual agents for colouring glasses blue. Of the two, cobalt is the more commonly used. It transmits a deep red band in addition to blue. Accordingly, if a cobalt blue is combined with a copper bluish green, the latter absorbs the red and an excellent blue glass is obtained. If, however, the colour required is to be midway between blue and green, as in signal lenses, copper is used in association with a small amount of iron.

Weyl observes that from the viewpoint of glass technology we need only consider cobalt compounds which are derived from the bivalent cobalt ion, as tervalent cobalt is not stable in the

FIGURE 1 (*above*) – '*The Annunciation,*' *from St Ouen, Rouen. Fourteenth century. An example of rich colouring in glass. The prominent yellow is from oxide or chloride of silver.*

FIGURE 2 (*right*) – *Figures of the prophets from the north aisle of the nave, Fairford. Late fifteenth century. The more subdued colouring of a late period window. The red is probably a flashed gold ruby.*

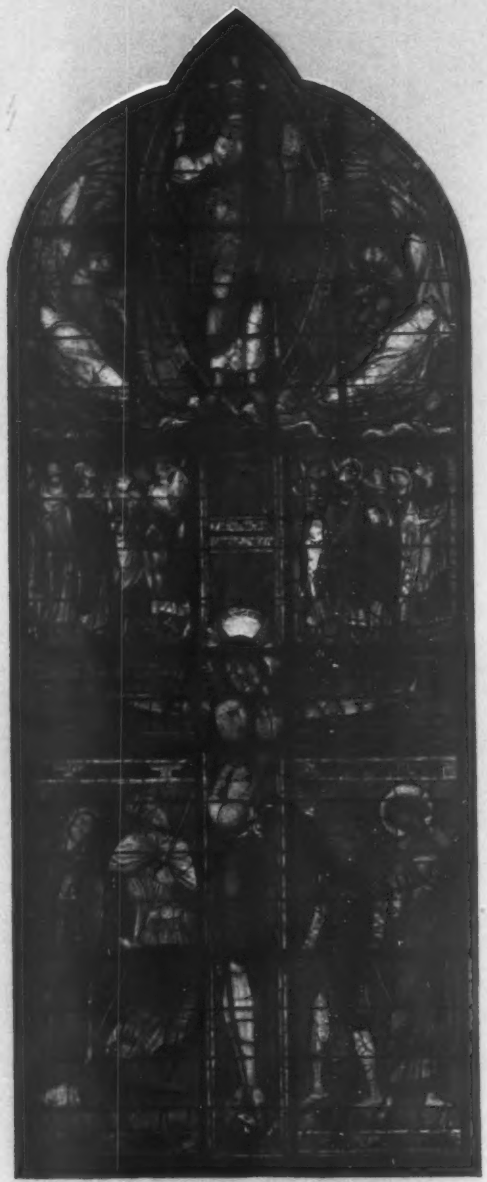

FIGURE 3 (*above*) – *Part of Crucifixion window, Poitiers. Late twelfth century. A brilliant polychrome in glass, showing the command which the glassmaker possessed over his materials even at this early date. Copper and manganese are among the colouring agents used.*

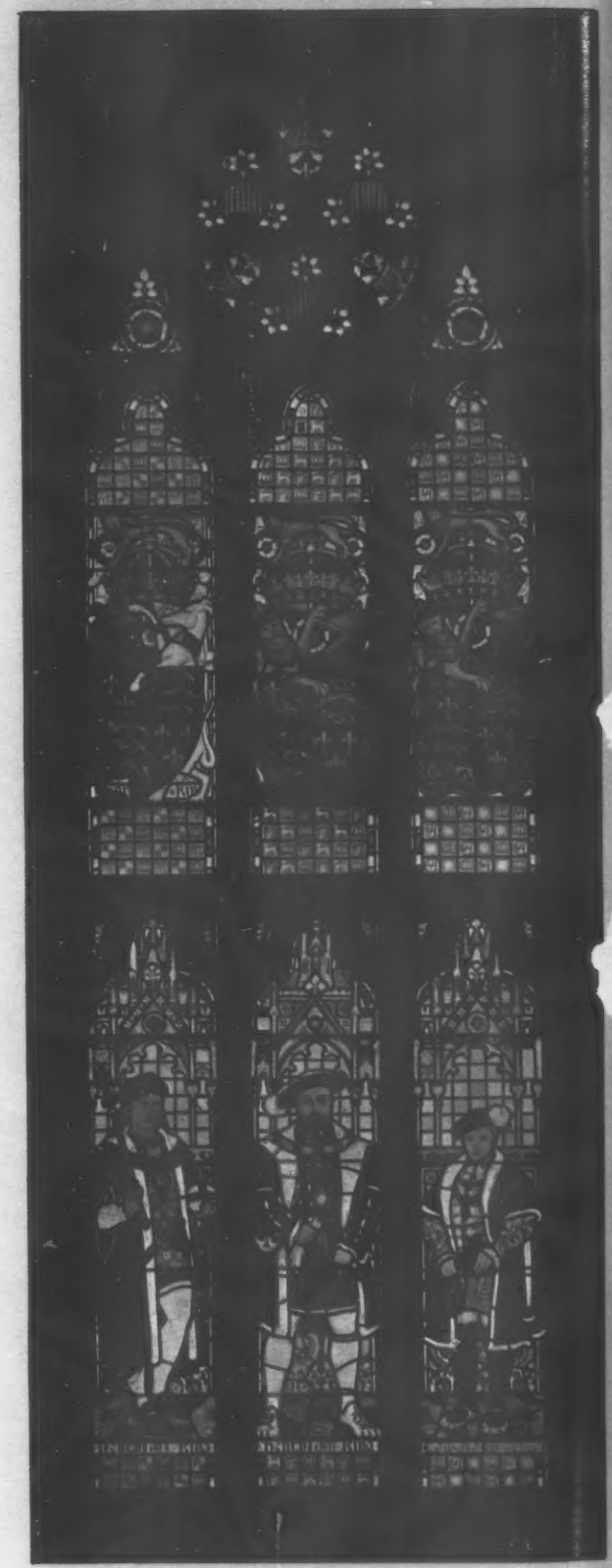

FIGURE 4 (*right*) – *A fine modern example of the use of stained glass for the decoration of a secular building. The red of the Tudor dragons is derived from selenium. The olive-green cloak of Edward VI (right) is from oxide of iron. From the Town Hall, Rochdale.*

FIGURE 5 (*above*) – *St Luke, from choir clerestory of St Ouen, Rouen. Fourteenth century. The rich ruby red of the mantle is probably from gold chloride. The object of the light grisaille staining is to allow more illumination than is obtainable through the richer but deeper colour of the central figure.*

FIGURE 6 (*left*) – *Colours in glass enable the pomp and circumstance of heraldry to be displayed to special advantage. The colour of the royal robes of Richard II (centre) is from cobalt oxide. From the Town Hall, Rochdale.*

FIGURE 7 (*left*) – *Bristol decanter. About* 1770–80. *An early form of wine decanter. The vivid blue is derived from copper.*

FIGURE 8 (*below, left*) – *Primitive glass vase, probably from Greek islands. In the attractive decoration of this vase the yellow colour is probably derived from antimony and the blue from copper.*

FIGURE 9 (*below, right*) – *Saracenic enamelled glass. Thirteenth century. This elegant vase is decorated with a blue which is a soda-copper silicate, and an opaque red from oxide of iron, the whole enriched with gold brocading to accentuate the design.*

temperature range required for glass melting. It has been found that potash glasses give a purer blue than the corresponding sodium glasses, and that borosilicates yield a reddish hue.

An interesting modern use of cobalt is in connection with glassware for lighting. If small amounts of cobalt, manganese, and copper oxides are used together in the batch, what is now known as the daylight effect is obtained. What happens is that these oxides eliminate part of the light from the artificial light sources, and the filtered remainder more closely resembles natural sunlight.

Sir Herbert Jackson suggested that the production and properties of glasses containing copper are representative of those in which the colouring is diffused in very minute particles throughout the glass, and which can be compared with colloidal solutions. In contrast to this, cobalt glasses are examples of those in which the colouring agents are in a state resembling solution, which might be compared to aqueous solutions of coloured salts.

Gmelin noted in 1779 that glasses containing iron became blue when founded under strongly reducing conditions, but no explanation was then possible. The colour was later attributed to a modification of ferric oxide which is stable only in the presence of ferrous oxide. This involves the existence of the two states of valency in the glass, or the simultaneous presence of FeO and Fe_2O_3. The addition of limestone to the melting glass may cause the colour to change to blue, as it may contain some material which acts as a reducing agent.

GREEN

For the production of green glasses iron, copper, and chromium are employed. Other possible agents, such as stannic oxide, are very rarely used.

The presence of 0·23 per cent. Fe_2O_3 gives an almost colourless glass which is greenish-blue in thick layers; 0·48 per cent. a light sea-green; 0·73 per cent. a bright sea-green; 1·23 per cent. a bright yellowish-green; 5·56 per cent. a deep yellowish-green, but still transparent; 8·23 per cent. a dark olive green, still transparent even in thick layers; 11·12 per cent. a dark olive green, but opaque in thick layers. The percentages stated were obtained by the analysis of test glasses, and therefore represent the amount of iron present in the finished glass.

As might be expected, the colours given by copper depend upon the conditions under which the glass is melted. A blue or green glass is obtained from copper compounds under oxidizing conditions and a ruby colour under reducing conditions. For green glasses the black or cupric oxide is used.

Chromium was discovered in 1795 in the Russian mineral crocoite, which is a lead chromate that has been used for colouring glass since the early part of last century. The name is derived from the Greek word *chrōma* (colour) and indicates that chromium compounds have vivid hues.

GREY

To produce grey or 'smoked' glass the colouring agents used are combinations of (*a*) cobalt, nickel, and uranium oxides, or (*b*) manganese, iron, and copper oxides. The glass may be of either the soda-lime or lead-potash type.

OPAL

Opal glasses fall into two main classes. First, that in which the opacifying agent dissolves, leaving the glass transparent while molten but causing it to become opaque upon cooling. Second, that in which the opacifier never wholly dissolves but remains very evenly and finely distributed throughout the molten metal. In the one type, the glass is quite transparent when gathered from the crucible and gradually becomes opal as it cools. In the other, the metal is already opal upon gathering but becomes more so as the temperature falls.

In most opal glasses the opacity is dependent upon the separation of either finely divided silica, or some metallic oxide, or both, from the mass of glass. The separation is assisted by the presence of fluorides and phosphates. A certain degree of opalescence may result from the separation of the fluoride (say as aluminium fluoride) or the phosphates themselves from the glass, but as a rule the batch is such that they remain combined as fluo-silicates and phospho-silicates, throwing the silica out of solution. The source of the opal colour in glass, therefore, may be either excess of silica, excess of a metallic oxide, e.g. of aluminium, tin, or antimony, or the presence of fluorides or phosphates.

Duval d'Adrian has sought to show that, by using the complex fluorides of silicon, boron, tin, zirconium, and titanium with the fluorides of the alkaline earths and heavy metals, and adding various mixtures of these to an ordinary glass batch, more satisfactory results are obtained than with simple fluorides.

Opal glass is being increasingly used for a wide variety of purposes. It is now commonly found in the form of illuminating globes and bowls, sheet glass, towel rails, imitation candle tubes for electric lighting, containers for pomades and other

druggists' sundries, and linings for electroplated articles.

ORANGE

According to Sir Herbert Jackson, the famous Chinese *sang-de-bœuf*, or oxblood, glazes were due to dispersed finely divided copper in amounts of the order of 0·5 per cent., the reducing conditions under which they were made preventing any tinge of green through the presence of cupric oxide.

PURPLE

The compounds of manganese are among the oldest colouring agents used in glass. Dralle and others have stated that the purple colour is produced by the tervalent manganese oxide, Mn_2O_3, in equilibrium with MnO. To secure a deep colour with manganese the founding has to be under oxidizing conditions, as reducing agents destroy the purple tint.

Two pieces of transparent glass of an intense purple colour were sent to the late Sir William Crookes from South America. They had been found on rubbish heaps at a high altitude and had been exposed to the action of the sun over a long period. The probable cause of the colour was manganese, which would be affected by the solar rays of short wavelength, present at an altitude of 4,000 metres but absent at sea-level owing to atmospheric absorption. The colour had penetrated the whole mass, but disappeared when the glass was reheated to softening-point. By exposure to radium rays the colour could quickly be reproduced.

RED AND RUBY

Copper oxide in the cuprous form, melted under reducing conditions, gives an excellent ruby. If a copper ruby is reheated slowly for some time the particles of copper aggregate and become visible to the naked eye, giving what is known as an aventurine glass. This type of glass was first made at Murano, the name 'aventurine' being from the Italian *avventurino* (chance), indicating its accidental discovery. It has been described as a semi-opaque glass filled with golden yellow spangles.

On the rare occasions when a gold ruby glass is made today it is generally prepared from a soft lead batch containing an excess of antimony and arsenic. The gold is added in the form of a solution of gold chloride, in the proportion of 1 oz. of metal to 200 lb. of batch.

Selenium was first isolated in 1817 by Berzelius, who used the mud of sulphuric acid plants as his raw material.

Although selenium is now being used more and more as a source of red colour in glass the colour is difficult to retain, because if great care is not taken the selenium has a strong tendency to volatilize.

VIOLET

Colours ranging from violet to purple can be obtained by adding less than 1 per cent. of titanium dioxide (TiO_2) to the batch mixtures of certain borosilicate or phosphate glasses.

YELLOW AND AMBER

It is believed at present that no single element can be used to isolate spectral yellow in glass. Glasses of varying shades of yellow are, however, produced by means of cadmium, silver, sulphur, and uranium.

Carbon is very commonly used in the manufacture of yellow glasses, being introduced in the form of coke, graphite, or anthracite. It is the sulphate-containing impurities in the carbon which are the true source of the colour. This was demonstrated among others by Splitzerber, who in 1839 melted two carbon-containing batches, the first of which had 1·75 per cent. of sodium sulphate but the other none, and only the former developed a yellow colour. Again, in 1865, Pelouze obtained similar results.

In the making of amber glasses it is usual to employ a mixture of sulphur and charcoal as the colouring material. The batch must of necessity be melted in a reducing atmosphere. Experiments by Fenaroli and others led to the conclusion that the elements of the group giving yellow colours are effective in colouring glasses only when they are present as polysulphides, polyselenides, or polytellurides.

It was in 1789 that Klaproth isolated a new element, uranium, from the mineral pitchblende. He obtained sodium uranate as a bright yellow precipitate, which soon came to be used as a colouring agent for glass and glazes. When available, uranium can be used with antimony to give a stable yellow in glasses containing lead.

ACKNOWLEDGMENTS

Figures 1, 2, 3, and 5 from *Stained Glass of the Middle Ages in England and France*, by Hugh Arnold. Paintings by Lawrence B. Saint. (A. and C. Black.)

Figure 7 from *English and Irish Glass*, by W. A. Thorpe (The Medici Society).

Figures 8 and 9 from *Glass*, by Edward Dillon (Methuen & Co.).

Plastic optical materials

D. STARKIE

Although the possibility of using transparent plastic materials in optics has long been recognized, the moulding of optical components presents much difficulty, owing largely to the tendency of the material to shrink away from the mould. This article includes an account of a method by which the surface of an original moulding can be built up so that the contours of the mould are faithfully reproduced. The method is of particular interest in connection with the construction of television receivers with relatively large screens.

From the time when plastics were first used in industry some of the transparent varieties appeared to be of value as possible new optical materials. A number of their physical properties were clearly favourable for optical work, e.g. low density, high light transmission, an outstanding toughness and resistance to severe thermal shocks, and an ability to reproduce faithfully the surface of a mould. On the other hand, when compared with glass, the transparent plastics have a relatively low resistance to scratching of the surface, less rigidity, and a relatively high coefficient of thermal expansion. Further, the range of plastic materials suitable for optical work is at the moment limited, although new transparent plastics providing an adequate range of optical constants will doubtless be synthesized and become available commercially. In attempting to assess the value of plastic materials to the optical industry, these advantages and disadvantages must be carefully considered.

THE VALUE OF PLASTICS AS OPTICAL MATERIALS

Two plastic optical materials are readily available in Britain, namely polymethyl methacrylate and polystyrene, of an appreciably higher purity than that normally associated with the commercial products. As a result of this increased purity, both materials have a relatively high softening-point and an improved craze resistance, and the development of new methods of manufacture and treatment has resulted in a freedom from strain and an optical homogeneity equalled only in the finest optical glass. The optical properties of these two materials are such that they can be used satisfactorily for achromatic lens systems. As they are pure chemical substances their refractive indices are unaffected by variations in the method of manufacture, and it is easy, therefore, to maintain the values accurate to 0·0001 from batch to batch—a factor of importance in precision optics.

The low density of the materials when compared with glass is attractive in that when plastic components are used in an optical instrument—particularly one containing massive prisms—a considerable saving in weight results. Their resistance to mechanical and thermal shocks has already made plastic optical materials valuable during wartime, and has made possible certain applications where glass is too brittle to use.

Extra care in designing an optical instrument can offset many of the disadvantages of the materials. The reduced rigidity calls for more attention in mounting if stresses causing distortion are to be avoided, although by reason of their low density plastic optical components, as compared with glass, require reduced mounting pressures to obviate displacement. The low scratch-resistance of optical plastic materials has tended to be over-emphasized, for a scratch on their surface is simply a depression with no broken edges, and the actual light-scattering area is appreciably less than with a scratch on glass. The relatively high thermal expansion again calls for care in design and mounting, and in extreme cases thermal compensating devices may be incorporated in the instrument. If the instrument is intended for use at very high temperatures it will be impossible to use plastic optical components, but a method of manufacture referred to as the 'surface-finishing' process, which has been developed recently [1] and will be described later, enables plastic optical components to be produced which are stable at temperatures up to 100° C.

It is, however, on the ability of plastic optical materials to reproduce faithfully the surface of a mould that the main interest has been centred, since it is this property which will enable the materials to make a new and extremely important

contribution in the field of applied optics. Given a method of highly accurate reproduction from a master mould, plastic optical components can be made rapidly and cheaply. No special difficulties in making large-diameter components will be encountered, and in consequence the serious difference of price existing between large and small components made in glass will not be paralleled. Moreover, components with non-spherical or aspheric surfaces can be made at a cost sufficiently low to enable them to be used for all optical applications, once the moulds have been prepared. The ability to produce optical components with aspheric surfaces at a relatively low cost represents the chief attraction of plastic optical materials.

ASPHERIC OPTICAL COMPONENTS

It is well known that a lens, the surfaces of which are portions of a sphere, suffers from spherical aberration. With a single lens forming an image of a point source of light, the aberration causes a spreading of the image over an area instead of a concentration of the light rays to a point. If accurate concentration of the light rays to a point is to be obtained by a single lens it will have to refract the rays striking its central portion more strongly, and the rays near its perimeter less strongly, than occurs in using surfaces of spherical form. For this reason the stopping down of a lens, i.e. the screening-off of the rays from the outer edges, produces a sharpening of the image, but of course at the expense of its intensity. If the full aperture of a single lens is to be used and an accurate point image is to be obtained, it is necessary to depart quite appreciably from surfaces of spherical form and to use lenses which are flatter near the edges and steeper at the centre than on a sphere.

It is possible to make some reduction in the amount of spherical aberration in a system of lenses by selecting the shapes of the individual lenses so that the spherical aberration introduced by the negative elements is as nearly as possible equal and opposite to that introduced by the positive ones. Many ingenious, but often expensive, ways of arranging lenses with spherical surfaces in a system to reduce spherical aberration have been devised, but they unfortunately impose added restrictions in the design of certain types of optical system. Further, any introduction of additional lenses to the system adds to the losses by reflection, owing to the extra surfaces. There are certain applications where it is impossible to obtain the requisite performance using lenses with spherical surfaces, and where only aspheric surfaces can be entirely successful.

Unfortunately, the usual grinding and polishing methods of making glass optical components normally allow only spherical or flat surfaces to be worked, and the sole satisfactory way of making surfaces other than these in glass is by laborious hand-figuring of small portions of the surface until the whole area of the component has been completed. The cost of hand-figuring an optical component in this way is very high indeed, for many days of patient work by a highly skilled craftsman are involved. The use of aspheric optical components has therefore been limited to applications where cost is of secondary importance only. Many attempts have been made to produce aspheric optical components at economic prices, e.g. by using machines of special design to work the surfaces automatically, but the impossibility of constructing machines capable of working to, and maintaining, the requisite degree of precision has prevented entirely satisfactory results from being obtained.

THE 'SURFACE-FINISHING' METHOD OF MANUFACTURING PLASTIC OPTICAL COMPONENTS

Two methods of making plastic optical components by reproduction from a mould are already in operation on a manufacturing scale. The first [2] is moulding the material by compression between the two halves of an optically worked stainless steel mould, the material having first been 'preformed' to the approximate shape of the interior of the mould by machining. The second [3] is by polymerization, in which the solid polymer component is cast from the liquid monomeric form, in the presence of a suitable catalyst, inside an optically worked glass mould. Optical components made by both these methods of manufacture (which have been in operation for a number of years) have found a variety of uses.

Details of the new 'surface-finishing' process for the manufacture of plastic optical components by reproduction from a mould have been published recently. This method promises to be of importance in that it gives an extremely accurate reproduction of the interior of an optically worked mould, while the resulting optical components are entirely free from strain and are therefore stable at high temperatures.

The main ideas on which the 'surface-finishing'

FIGURE 1 – *Two aspheric lenses with their respective moulds.*

FIGURE 2 – *An 8 in. diameter optical system for a domestic television projection receiver, and an 18 in. diameter system for television projection in a cinema.*

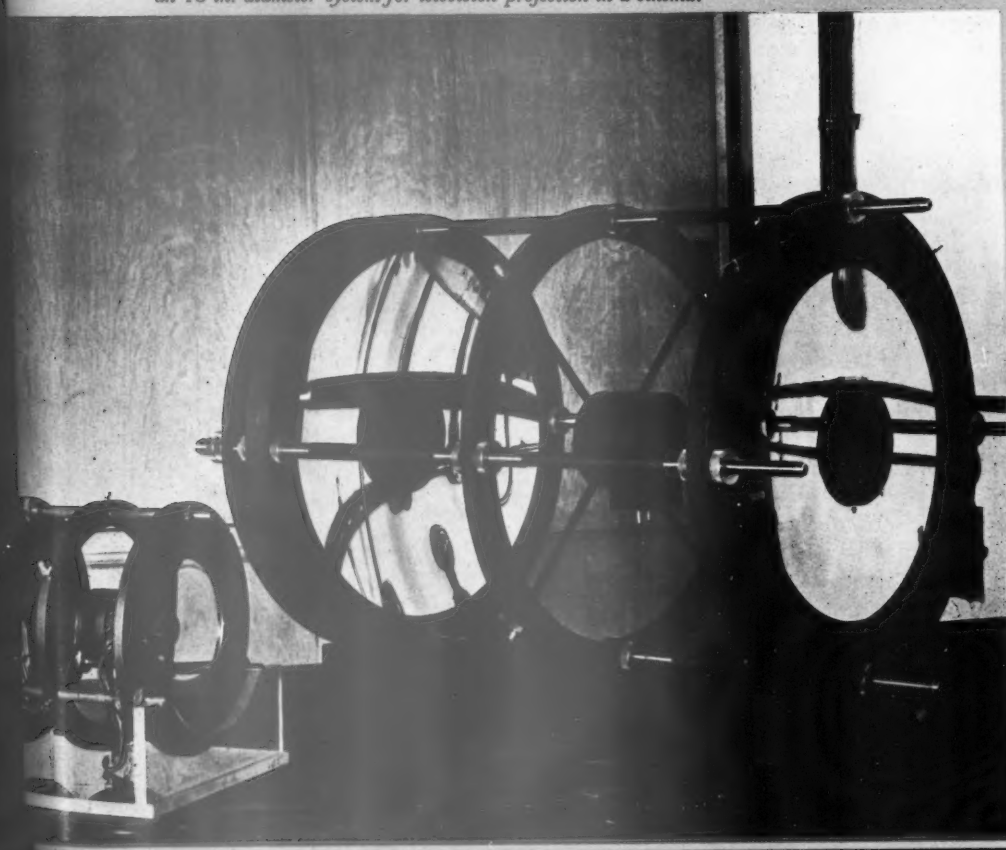

FIGURE 3 – *An 8 in. diameter television projection system projecting a picture on to a separate screen.*

process was based were (*a*) to make a strain-free preform of an optical component by some known method, and then (*b*) to build up on the surface of this preform a thin skin of the same material inside an optically worked mould until its surface made optical contact with the mould at every point. After stripping from the mould, the optical component would then be ready for immediate use. It was assumed to be essential that the use of pressure throughout the reproduction process should be avoided if perfectly strain-free, and hence stable, optical components were to be obtained.

After considerable investigation, straight polymerization, or casting from the monomeric state, in a mould, such as is used in one of the existing processes of manufacture already mentioned, was selected as the method of making the preforms. For a variety of reasons, and particularly with a view to easy operation on a manufacturing scale, polymerization by exposure to light, with a suitable photo-catalyst present in the monomer, was adopted in preference to the more usual heat polymerization for making the preforms. A considerable portion of the investigation has been concerned with the discovery of photo-catalysts and satisfactory light-polymerization conditions. There is a large contraction in volume with all the most desirable optical plastics on changing from monomer to polymer, and this increases the difficulty of making an accurate preform; but the contraction can be reduced to some extent by carrying out a partial polymerization of the monomer before filling the mould. The liquid which finally goes into the mould has a high viscosity, and the degree of polymerization of the liquid which can be allowed is limited by the fact that it must flow sufficiently easily to fill the mould. The final contraction inside the mould is taken up by allowing the two halves of the mould to be drawn together during the polymerization. When polymerization is complete, the mould containing the preform is chilled suddenly, and the two become separated owing to the fact that they do not contract equally. The preform, as it leaves the mould, is surrounded by 'flash,' which is machined off,

after careful optical centring, in a lathe fitted with a vacuum chuck. The last stage in the preparation of the preform is a careful annealing to remove all traces of strain which may have been introduced by firm attachment to the surfaces of the mould during polymerization.

The difficulty in controlling the relatively large contraction in volume on polymerization makes it practically impossible to obtain first-class optical accuracy in the preform, and if the latter is placed back in the mould it will be found to have departed slightly from the true form. These minute spaces are filled up, therefore, by polymerizing a thin film of the same material on the surface of the preform, the film being so thin that its contraction on polymerization can be accurately controlled. Great care in carrying out this operation is required, and an exact routine procedure has been worked out. After stripping from the mould, the finished optical component is given a final edging on a lathe fitted with a vacuum chuck.

If optical mirrors are being made, a reflecting layer of aluminium is deposited on them by evaporation in a vacuum chamber, and the same apparatus can be used for coating lenses with non-reflecting films.

All the moulds used for the 'surface-finishing' process are made of glass. Glass was selected as being the hardest suitable material available in sufficient quantity. Since part of the reproduction process is by light-polymerization a transparent mould material is, of course, required.

The time necessary to make an optical component by the new process is long, although the actual handling time is short. A suitably equipped factory would contain mechanical conveyors to carry a large number of moulds through the various stages of the process, and it is hoped that such a unit will be in operation in Britain in 1948.

APPLICATIONS REQUIRING ASPHERIC OPTICAL COMPONENTS

The 'surface-finishing' process is to be applied in the first place to the manufacture of aspheric optical components, and the workers engaged on the development of the process have had to investigate both the design of such optical systems and methods of working aspheric moulds. Much progress has been made in each direction. As regards the working of the moulds, it was accepted from the beginning that hand-working methods would have to be used, and the main concentration was on devising new methods of testing the exact contours of the mould at all stages of its preparation, and on providing mechanical devices which would be helpful to the skilled craftsmen engaged on the work. New mechanical methods of testing which can be operated by the optical worker himself are used in the early stages of the mould-making, while optical tests carried out by experienced men are employed in the final stages. Complicated aspheric moulds are now produced at a much higher rate than was anticipated in the early days of the investigation. Figure 1 shows two aspheric moulds, together with lenses made from them.

An application for which an aspheric optical system is essential is television projection. In view of the urgency with which television projection systems are likely to be required, the 'surface-finishing' process has been applied primarily, up to the present, to the preparation of a range of prototypes for this application. It is generally agreed that the picture on the curved end of the cathode-ray tube which is provided by the present domestic television receiver is by no means satisfactory for viewing by a number of people, and that a much larger, flat picture is desirable. A sufficiently large cathode-ray tube would be bulky and expensive, and a much more attractive way of providing a picture of the desired dimensions is to generate a high-intensity television picture on the end of a small tube and to project an enlarged image of it on to a separate screen by means of a suitable optical system. Unfortunately the brightness of the cathode-ray tube picture in a domestic receiver is necessarily limited, and to give a satisfactory screen picture an optical system of large light-gathering power is required. A system capable of meeting all requirements is one that was designed originally for use in an astronomical telescope [4] and is usually referred to as a Schmidt system. In applying such a system for television projection, the small cathode-ray tube faces a concave mirror of large aperture which collects and projects beyond the tube a considerable percentage of the light from the television picture, while an aspheric corrector plate situated at the centre of curvature of the mirror corrects the spherical aberration of the mirror and redirects the rays of light accurately to their appropriate positions on the screen. A drawing of a Schmidt system used for television projection is shown in figure 4. With this combination of mirror and corrector plate, f numbers of 0·75 can be obtained without difficulty, even though the cathode-ray tube screens off the centre

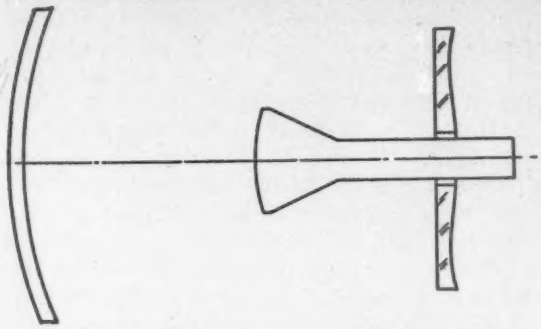

FIGURE 4 – *Diagram of a Schmidt system used for television projection.*

of the system. Provided that the mirror and corrector plate are of sufficient accuracy, a bright, sharply defined screen picture can be obtained from a relatively low-intensity cathode-ray tube picture. Once the moulds have been prepared, both mirror and corrector plate can be made by the 'surface-finishing' process at only a fraction of the cost of the same system in glass.

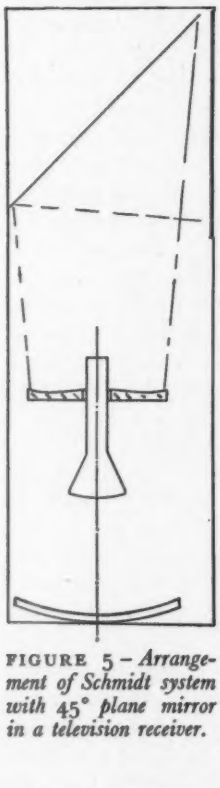

FIGURE 5 – *Arrangement of Schmidt system with 45° plane mirror in a television receiver.*

Up to the present, Schmidt systems for two sizes of projection cathode-ray tube for use in domestic receivers have been made by the new process. One system, containing an 8 in. diameter mirror, is for use with a $2\frac{1}{4}$ in. diameter cathode-ray tube, while the other, with an 11 in. diameter mirror, takes a $3\frac{1}{2}$ in. diameter tube. Both systems have a focal ratio of $f/0{\cdot}75$, have a throw-distance from mirror to screen of 40 in., and give a screen picture measuring 15 in. by 12 in. Figure 5 shows how, by the introduction of a plane mirror at an angle of 45° to the light beam and a translucent screen, this 'throw-distance' of 40 in. can be accommodated in a television cabinet of reasonable dimensions. Figure 3 shows the 8 in. diameter system in operation, projecting an actual picture on to a separate screen.

There is also a demand for full-size television projection in cinemas. Here the life of the cathode-ray tube is less important than in the domestic receiver, and expensive auxiliary equipment can be installed, so that a cathode-ray tube picture of much greater brightness can be provided. This greater brightness in the tube picture is, however, offset by the fact that the projected image covers a very much greater area on the screen, and an optical system of large light-gathering power is again required. A Schmidt system of $f/0{\cdot}8$ containing an 18 in. diameter mirror and with a throw-distance of 42 ft. has been made by the 'surface-finishing' process. This system, which is being used experimentally in a cinema, is giving a picture 10 ft. by 8 ft., and the brightness of the screen picture is equal to that normally obtained in a cinema by projection from a film. An optical system of still greater dimensions for use in the largest types of cinema is now being prepared. The comparative sizes of the 18 in. diameter cinema system and the 8 in. diameter domestic system can be judged from figure 2.

Although the 'surface-finishing' process has so far been applied primarily to optical systems for television projection, a wider field of application has been borne in mind. Prototype aspheric magnifying lenses which allow an operator to inspect tiny manufactured parts (using both eyes) without experiencing any sign of eyestrain have already been made. Projection systems of large light-gathering power for applications requiring either high-intensity projected pictures for viewing in brightly illuminated surroundings, or reasonable picture brightness from low-wattage lamps, are being designed, and practical trials will be carried out in the near future. These applications include filmstrip projectors for educational and publicity purposes, new types of signals and indicating devices for railways, micro-film projectors, and special projectors for use in film studios. All these systems and components, which may be required in substantial quantities, are particularly suitable for manufacture by the process of 'surface-finishing,' which will allow plastic optical materials to play an important part in the field of applied optics in the future.

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The story of malleable platinum

M. SCHOFIELD

The intractability of platinum long obscured its usefulness, and to the early Spanish miners it was nothing more than a tiresome impurity, to be discarded and dumped into the rivers of South America. Realization of the valuable properties of platinum was followed by a long struggle to render the metal malleable so that it could be fabricated into useful articles. The subsequent extensive use of platinum for laboratory and industrial equipment, jewellery, dental accessories, and other articles was a first result of pioneer work in powder metallurgy.

The history of platinum, otherwise *platina* or 'white gold,' may be divided into two parts, each of which is as absorbing as that of any other metal. There is the general history of the element, well known and set in order: a tale opening with such names as that of Julius Caesar Scaliger who, in 1557, made a first reference to a metal found between Mexico and Darien, a metal 'which no fire nor any Spanish artifice has yet been able to liquefy.' Then there is the second section, hardly as yet fully knit together: a section dealing with the making of malleable platinum, with a process which put into service a metal of which whole cargoes had lain idle as useless. This second part is indeed compelling, for it brings on the scene so many chemists of Europe: men like Scheffer, Macquer and Baumé, Achard and Lavoisier, and the less-known Chabaneau, with a goldsmith or two like Jeanety of Paris. Most important of all, it brings in William Hyde Wollaston, Thomas Cock, Richard Knight, Johnson, and Matthey to finish the early history of platinum fabrication on a triumphant note for the British school. But even today there are gaps and disjointed pieces in this story, particularly as regards the making of large platinum vessels for the concentration of vitriol. Had Wollaston sufficient elbow-room wherever he worked to construct those platinum vessels which the *Dictionary of National Biography* maintains he made himself? How much knowledge did he gain from Thomas Cock? Certain it is that the platinum still or boiling-vessel used by Sandemann was not made by Johnson, Matthey and Company as some historians have suggested, for, as D. McDonald [1] has pointed out, in 1809 P. N. Johnson was but sixteen years old and George Matthey had not been born.

William Hyde Wollaston, M.D.

In studying the fabrication of platinum, distinction must be made between early workers using, intentionally or otherwise, an alloying element to render platinum more susceptible to mechanical working and forging, and the first workers beginning the powder metallurgy of pure platinum. Among the latter Wollaston was a dominating figure. Long before Don Antonio de Ulloa, a member of the 1735 French and Spanish expedition dispatched to measure a degree of meridian at Quito, published his *Historical Account of the Voyage to South America*, primitive Indians had fished out from the Pinto river some water-worn nuggets and grains of this 'free' metal (free for anyone to take), and fashioned it into ornaments. According to Bergsøe [2] these Indians, in fashioning their nose-rings, could not have melted platinum. Not a single cast object was found, nor was any solder used. All was done by welding and cold hammering, using charcoal and the blowpipe to bring the grains into a coherent form. But there was 30 per cent. of gold present to seal together the platinum grains before the mass was sintered and then made homogeneous by hammering and

heating. Such alloying of platinum and gold has persisted through the centuries right up to the present day: a modern example is the platinum-gold spinneret to which rayon producers have reverted after tantalum—rival to platinum—failed to stand the test. Yet platinum was to the gold-miners of the Choco district of Columbia what tungsten was to the tin-miner, or nickel ('Old Nick's metal') was to copper-miners. Gold mines were abandoned because of it, and, since the Spaniards were not averse to using gilded platinum as counterfeit, their government banned its export and ordered the metal to be dumped into rivers.

Though the ship bringing back De Ulloa was captured by the British Navy, the Admiralty 'unanimously and with pleasure' returned to him his scientific manuscripts, adding that they were 'not at war with the arts and sciences or their professors.' Hence his historical account was saved, and after its publication just two centuries ago all sorts and conditions of chemists in Europe were attracted to 'this obdurate body.' De Ulloa went on to develop the Almaden quicksilver mines, leaving it to Sir William Watson and William Brownrigg to introduce platinum. Brownrigg [3], whose *platina* from Jamaica had been given him by a relative, Charles Wood, told of its high melting-point and its refractoriness (noted by Wood), and he believed the Spaniards had melted it and cast it into sword-hilts, buckles, and snuff-boxes. Such first dabblings with platinum were extended by the Swede Scheffer [4], who in 1752 succeeded in fusing a sample of 'white gold' (obtained from Watson in London) by the use of arsenic; and by Lewis [5], whom Scheffer anticipated and who published his observations in the *Philosophical Transactions*. In 1772 von Sickingen rendered platinum malleable by alloying it with gold and silver. He also dissolved it in *aqua regia*, added ammonia, ignited the precipitated ammonium platinichloride, and hammered the fine platinum to effect cohesion.

Such an important step towards producing malleable platinum by way of the metallic powder was the practical realization of an idea of Antoine Baumé, who suggested that platinum could probably be forged and welded as iron had been worked among the ancients. In mentioning Baumé one must give credit to several Frenchmen, chemists and others, who made persistent efforts to harness the metal. Thus M. de l'Isle [6] in 1770–4 used the ammonium platinichloride method to obtain finely divided platinum, and agglomerated this to the malleable form. De Morveau [7] tried alloying with lead as well as the arsenic method. Achard, father of the beet-sugar industry, also used arsenic and is said to have made the first platinum crucible. There was also the Abbé Rochon [8], director of the Marine Observatory at Brest, whose method was to purify platinum grains 'in a strong fire' with nitre and sulphate of soda, adding one-eighth part of the tin–copper alloy used for common specula, exposing the mixture to 'violent heat,' and fusing it before pouring it into a mould to give a large speculum weighing 14 lb., with a diameter of 8 in. and a focal length of 6 ft. To continue the part played by Frenchmen, Lavoisier [9] in his *Observations sur le platine* told how Baumé tried lead and bismuth as alloying metals; others used arsenic, which was removed by vaporizing on subsequent heating; and Rochon used copper and tin capable of being dissolved in nitric acid after reducing, leaving platinum as a black powder. Lavoisier doubted whether this could be used in large-scale work, and then referred to Jeanety as the man worthy of most merit. The jeweller Jeanety of Paris, receiving some platinum ingots from Chabaneau, made platinum snuff-boxes, a coffee pot shown by Lavoisier to the Academy, watch chains, and platinum buttons. Berthollet and Pelletier credit Jeanety with perfection in platinum-working by alloying with arsenic in a crucible, obtaining a button, breaking this up and treating the product with white arsenic and refined potash, and finally vaporizing the arsenic—an eight-day operation. To Jeanety, let it be noted, came the *savants* to buy platinum crucibles and the Academy to get bars of platinum.

Jeanety's platinum was supplied by Pierre François Chabaneau, that versatile yet almost unknown French chemist, who deserves a paragraph to himself. While Jeanety was the first to make fairly pure platinum sponge in quantity and to fabricate it at a white heat, he continued with his arsenic when he failed to discover Chabaneau's method. Chabaneau [10], a native of Dordogne, was expelled from his theology school and took a mathematical teaching post at Passy, where he studied by firelight for the next day's teaching. It was while he was giving some public lectures that certain Spanish members of the audience were attracted by him and persuaded him to go to Spain. So we hear of Chabaneau going to Vergara to teach physics, collaborating with de Elluyar of tungsten fame, and being installed by Charles III in a science chair at Madrid, with a royal palace as lodgings and a valuable library and well-equipped laboratory at his disposal. At this period Spain suddenly

realized that her South American platinum, hitherto despised, was an asset. It was turned over *en masse* to Chabaneau, thus beginning what has been called 'the platinum age' in Spain. Chabaneau had a difficult problem: chemistry was not yet sufficiently advanced to bring the recognition that platinum could be malleable at times and brittle when iridium was present, and that the original ore contained a number of metals like gold, mercury, lead, copper, and iron as well as those of the platinum group. 'Away with it all! I'll smash the whole business!' cried Chabaneau on more than one occasion as he threw his apparatus out of doors in a tantrum. 'You'll never again get me to touch the damned metal!' Yet it was a different Chabaneau who one day placed on the table of the Marqués de Aranda a 23 kg. ingot of malleable platinum in the shape of a 10 cm. cube—which the Marqués found surprisingly heavy. Chabaneau and his fellow-worker Cabezas made utensils and ingots in their prosperous business, and produced platinum crucibles for the use of Joseph Louis Proust. The king would often come to the laboratory to watch Chabaneau at work, but what was more important was the grant of a life pension and letters patent of 1783 establishing priority for Chabaneau's method.

While the goldsmith Jeanety continued to supply platinum apparatus from the metal processed by the Scheffer-Marggraf-Achard method, involving the use of arsenic and its subsequent volatilization, and while Count Apollos Moussin-Poushkin [11] in 1805 was using a mercury amalgamation method with the removal of mercury by the same means, the English school of platinum fabrication was developing. Richard Knight [12], for example, published a *New and Expeditious Process for rendering Platinum Malleable*. This involved compression of ammonium platinichloride into a hollow conical crucible with cover, the reduction to platinum at a strong white heat in an air furnace, and the hammering-in of a red-hot stopper to yield a platinum button which was then hammered and further heated. There was also Thomas Cock [13], who reduced platinum to a sponge in the platinichloride process and followed this by compression in an iron mould with a screw press for removing air. When volatiles had been driven off at a white heat, the platinum was hammered 'in all directions' on an anvil, and after a borax-flux treatment the working was repeated. Cock was a brother-in-law of P. N. Johnson and worked in William Allen's laboratory at Plough Court, London. He is believed to have worked on platinum from 1800 to 1809; he then disappeared for a time and later is heard of helping his brother-in-law and leaving a son to carry on the business. He seems to have communicated his method to Wollaston at Allen's request [14] and to have rediscovered Chabaneau's process—as did Knight (the latter, according to one report, being the first to fabricate large sheets of pure platinum free from flaws [15]).

Yet among the early platinum-workers William Hyde Wollaston stands out as the most prominent figure. There is a hint that Wollaston [16], after seeing Cock's work, took away some ideas which he did not acknowledge—ideas extra to whatever Cock told him. Whatever the truth of this may be, his own contribution is so great, as D. McDonald [17] remarks, that anything Cock provided could have been only a minor part. Wollaston, working in his secret laboratory, was the first to fix the composition of native platinum, and was the collaborator of Smithson Tennant in the study of palladium, osmium, iridium, and rhodium. He knew the exact proportions of *aqua regia* mixtures which would leave iridium undissolved, and how to expel volatiles from the platinichloride without allowing the platinum particles to adhere. But he was a business man and made a fortune of £30,000 from platinum fabrication, using it as a means of retiring at 34 to devote himself to scientific studies. He seems to have obtained platinum on a fairly large scale, fabricated it into laboratory ware (thus making this country independent of manufacturers abroad), and then turned his attention to making those large boilers for sulphuric acid which could not have been made by Johnson and Matthey. Wollaston was secretive and published nothing until, just before his death, he decided to broadcast his method in the Bakerian Lecture [18] of 1828. 'As, from long experience, I probably am better acquainted with the treatment of Platina, so as to render it perfectly malleable, than any other member of this society . . .' —so he opens his dissertation in which he tells how he avoided dissolving iridium by dilution of the *aqua regia*; how he rubbed the finer grey platinum powder in his hands and the coarser in a wooden pestle and mortar so as to avoid burnishing the particles. He washed the powder by levigation, made a mud of specific gravity 4·3 with water, compressed this to a cake of specific gravity 17–17·7 in a conical brass barrel-mould greased with lard, and used a lever press in place of Cock's screw press. In Wollaston's hands every part of the process was subjected to minute care and attention. He used first a charcoal fire to remove

moisture and grease and give finer cohesion, and then a wind furnace fired with Staffordshire coke to effect cohesion before hammering, reheating, and forging. As Thomas Thomson [19] put it, 'Wollaston first succeeded in reducing platinum into ingots in a state of purity and fit for every kind of use.' With his warnings against work-hardening the powder, Wollaston was an early powder-metallurgist, a pioneer in that struggle which has given us tantalum, tungsten, molybdenum, and niobium.

Wollaston's success had its fruits both in the laboratory and on a large scale. Instead of relying on France and Jeanety's followers for apparatus, chemists like Berzelius would apply to London for malleable platinum, and later, after Wollaston's method was published, were enabled to renew platinum apparatus made by less satisfactory methods.

On the commercial side it was for concentrating vitriol in stills or boilers that malleable platinum was brought to prominent use. It was Sandemann, a chemist member of the British Mineralogical Society, who maintained a small chemical works at Thames Bank and used such large boiling vessels, although Richard Farmer seems to have been the first sulphuric-acid maker to adopt this process, according to Knight's *English Encyclopaedia* of 1857 [20]. At his works on Kennington Common, Farmer engaged Wollaston to supervise the construction from his own platinum of a 323½ oz. troy vessel at a cost of £300; other vessels followed in 1820 totalling 828 oz. and costing £685. Wollaston charged for the weight of platinum supplied and for the workmen's wages, his own services being free. Sandemann in December 1809 had a vessel made of deep circular form, the weight being 423 oz. and the capacity 300 lb. of sulphuric acid, for use under a lead-chamber plant. This was the vessel previously referred to, which could not have been made by Johnson and Matthey.

With the death of Wollaston other makers of malleable platinum were left to carry on in London, Paris, and Russia. In France there were Desmoutis and Quennessen. In London were P. N. Johnson, founder of Johnson, Matthey and Company, who worked with Cock, his brother-in-law, with W. J. Cock the son, and others in business as Johnson & Sons. Johnson and Matthey soon won first place among such producers, and kept it through the agency of George Matthey and John Scudamore Sellon, having access to the considerable resources of Russia for their raw material. Half-way through last century the British concern had captured the world market, all this following a fruitful collaboration between Matthey and Sellon on the one hand and Deville and Debray [21] on the other. In 1861 platinum boilers for treating sulphuric acid were shown at a London exhibition, these being made by a 'patent autogenous soldering.' More than 160 concentrating vessels of from 20 to 160 gallons' capacity had been made by the old system of gold-soldered joints, but the patent soldering used the oxy-hydrogen blowpipe method as first described by Robert Hare before the Chemical Society of Philadelphia in 1801. In 1878 at the Paris Exhibition [22] Johnson and Matthey exhibited platinum ware, Deville and Debray's melting-process having replaced the compression-hammering method in producing such large ingots as were then prepared. 'Sponge' ingots are superior to unalloyed melted metal, provided the sponge is pure and that all the precautions given by Wollaston are observed. Such is the early story of malleable platinum. Further important advances in the metallurgy of the platinum metals have of course been made since those pioneering times, but they lie outside the scope of the present article.

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The metabolism of nitrogen in soil

J. H. QUASTEL

From the bland assurance and circumstantial detail with which many elementary textbooks present the 'nitrogen cycle,' students might be pardoned for believing that the problem had been solved once and for all. Professor Quastel has a very different story to tell. He shows that the study of nitrogen metabolism in soil is still in its comparative infancy, and that while many of the basically significant facts have been elucidated there are very numerous phenomena of great interest—and great difficulty—yet awaiting full investigation.

Nitrogen metabolism in soil covers the vast sequence of chemical changes undergone by the nitrogen molecule in biological processes taking place in soil, from its initial fixation until its final liberation into the atmosphere. The main processes comprising this cycle of changes are as follows:

1. *The transformation of atmospheric nitrogen by soil micro-organisms into substances nourishing microbe and plant.* This process is generally known as biological nitrogen fixation, and is accomplished largely by two sets of organisms, one consisting of the root-nodule bacteria living in symbiosis with certain leguminous plants, and the other consisting of organisms living in the soil independently of plants.

2. *The transformations in soil of organic nitrogen compounds arising from autolysis of all forms of biological material, or from the excreta of animals, or from the products of metabolism of living soil organisms, into ammonium ions.* These processes are accomplished by a great variety of soil micro-organisms which, while developing aerobically or anaerobically on organic nitrogen compounds, produce ammonium ions as part of their metabolism; or they may be brought about by the action of hydrolytic or oxidizing enzymes present in soil micro-organisms, whether they are proliferating or not.

3. *The conversion of ammonium cations into nitrite and nitrate anions.* This process is generally referred to as soil nitrification, and is accomplished largely by two groups of organisms, *Nitrosomonas* (also *Nitrosocystis* and *Nitrosospira*), which forms nitrate from ammonium ions, and *Nitrobacter* (also *Nitrocystis* and *Bactoderma*), which converts nitrite ions into nitrate ions.

4. *The reduction of nitrate into nitrite and finally into ammonium ions or into free nitrogen.* Reduction of nitrate into nitrite and ammonia is accomplished by many micro-organisms, and is a process which has been much investigated. The biological process of nitrogen production, or of formation of oxides of nitrogen, from nitrates and nitrites, is, however, not so well understood. Nitrates form the main source of nitrogen for plant life growing in soil, and therefore ultimately for all animal life.[1] The nitrogen returns to the soil largely in organic combination, and undergoes the changes mentioned in process 2. Nitrogen is not lost to the atmosphere in its transformations in the intact plant or animal.

The total combined nitrogen on earth would increase, as a result of the biological catalytic processes of soil which fix atmospheric nitrogen, were it not for the fact that mechanisms exist which provide for the release of free nitrogen from combined forms such as nitrite or ammonia. The process whereby reduction of nitrate (or nitrite) takes place to form gaseous nitrogen (or oxides of nitrogen) is known generally as denitrification.

BIOLOGICAL NITROGEN FIXATION

It is now known that certain specific micro-organisms working symbiotically with legumes can fix atmospheric nitrogen. The following is a brief history of this discovery.

Boussingault in 1837 carried out the first quantitative field and laboratory experiments, indicating that fixation of atmospheric nitrogen takes place during the development of legumes such as clover, peas, and lucerne, whereas no such fixation occurs during the growth of such crops as wheat or oats. Liebig (1843, 1852) opposed the view that free nitrogen of the atmosphere is assimilated by the plant. He considered that the beneficial effects of the legumes are due to their large leaf surfaces, which offer greater areas for absorption of atmospheric ammonia. Ville (1855), however, showed that atmospheric ammonia is insufficient in quantity to account for the observed increase of fixed nitrogen in the development of legumes. The subject remained in a confused state until after the

[1] Ammonium nitrogen is, however, assimilated by many plants growing in soils which do not support ready nitrification, e.g. certain grasses and forest trees.

work of Lawes, Gilbert, and Pugh at the Rothamsted Experimental Station. In 1851 these investigators began experiments in an attempt to explain the results of field trials which had extended over sixteen years. The data had demonstrated that plots cropped to non-legumes without addition of manures give rise to only low yields, while plots cropped to legumes maintain high yields even without any manurial treatment. Moreover, if a non-legume follows a legume in a rotation, the yield is as high as if the field has been previously in fallow for a year. This occurs in spite of the fact that a large quantity of nitrogen is removed in the preceding leguminous crop. Liebig's explanation of absorption of atmospheric ammonia was found to be erroneous, partly because too small quantities of ammonia are absorbed from the air and partly because legumes and non-legumes behave very differently.

The logical deduction appeared to be that legume development in the field is accompanied by fixation of atmospheric nitrogen. Careful pot experiments, however, by Lawes and his colleagues (1861) gave rise to the conclusion that no fixation of molecular nitrogen takes place during the development of either legumes or cereals. The view was therefore held, and accepted for the following twenty-five years, that plants cannot fix atmospheric nitrogen. The clear-cut results of the Rothamsted field experiments still remained unexplained. The classical work of Hellriegel and Wilfarth in 1886 eventually clarified the situation. They were able to show that certain bacteria in the soil infect legumes, forming nodules which enable the plants to use atmospheric nitrogen; these bacteria have no infective power on cereals.

The organisms responsible are known as root-nodule bacteria or *Rhizobia*. They were first isolated in pure culture by Beijerinck.

NITROGEN FIXATION BY *RHIZOBIUM*

Little is known as yet of the mechanism of nitrogen fixation by the root-nodule bacteria in association with the plant. Neither the host plant nor *Rhizobium* can independently fix nitrogen, except under very specialized conditions which are under investigation. It is obvious that there must be some chemical interchange between plant and bacteria, taking place *in vivo*, which is largely responsible for nitrogen fixation. Before considering this chemical interchange, a few of the biological aspects of the association between *Rhizobia* and host plant may be mentioned.

When the seed of a legume develops in a soil containing *Rhizobia* the latter are attracted to the region of the developing root hairs. The result of the presence of the *Rhizobia* in the near neighbourhood of the root hairs is to produce a 'curling' or deformation of the hairs. Specific chemical substances are responsible for this 'curling,' for bacterial extracts in absence of *Rhizobia* are found to be as effective as the organisms themselves. The response is biologically nonspecific and may be induced by extracts of bacteria other than *Rhizobia*. At the site of deformation of the root hair, *Rhizobia* invade the root tissue and proliferate within the hair in the form of a thread directed towards the cells of the root. The migrating bacteria are embedded in a gum, and an infection thread is developed. A sheath is laid down by the host cells around the embedded bacteria. Cell division is stimulated, the newly formed tissues are invaded by more bacteria, and thus a nodule is formed. Thimann (1936) concludes that the *Rhizobia* produce in the root hair a specific compound, probably indoleacetic acid, which is responsible for the local stimulation of the host cells. Both the bacteria and the host cells fail to proliferate as the nodule becomes older, and finally the tissue becomes necrotic. The nodule softens, its interior is digested, and it finally sloughs off. The *Rhizobia* return to the soil.

There is evidence that the deformation of the root hair, essential as a preliminary to the invasion of the *Rhizobia*, is also accomplished by indoleacetic acid, a common metabolic product of bacteria. It is known that indoleacetic acid and related plant 'hormones' have but a transitory existence in soil, owing presumably to speedy decomposition by bacteria. It follows, therefore, that metabolic conditions in the soil in the near neighbourhood of the germinating legume must be such as to enable a sufficiently high concentration of indoleacetic acid to accumulate to bring about the physiological response in the root hairs. Though such a concentration may be small—of the order of one part in 100,000,000—a relatively high rate of production may be necessary to counteract the local destructive forces as work. It is known, too, that indoleacetic acid at low concentrations may be toxic to seed germination, and that tryptophane in presence of soil bacteria will induce toxicity. Thus metabolic conditions in soil will greatly influence seed germination.

The presence of combined nitrogen (e.g. nitrates or ammonium salts) in the soil in which the host plant is growing apparently makes the plant resistant to attack by *Rhizobia*. It has long been

known that nitrates impede the development of nodules on legumes: fewer root hairs are deformed and fewer nodules are formed.

Light may be thrown on the chemical association between *Rhizobia* and host plant by the recent discovery (Kubo, 1939) that haemoglobin exists as a red pigment in the nodules of legumes. Keilin and Wang (1945) have confirmed this discovery, finding that the pigment of soya-bean nodules is a haemoglobin with a characteristic absorption spectrum.

Although haemoglobin is very widely distributed in nature its presence has been considered as general only in vertebrates. This view may have to be seriously modified by the discoveries that the pigment exists in a ciliate *Paramoecium* (Sato and Tamiya, 1937) and in the root-nodules of legumes. Neither the plant cells alone nor the *Rhizobia* grown separately synthesize haemoglobin. 'It is only when root cells are invaded by a specific symbiotic micro-organism and begin to proliferate that haemoglobin is formed. *Rhizobium* not only induces growth and multiplication of cells but also supplies these proliferating cells directly or indirectly with a factor necessary for synthesis of haemoglobin' (Keilin and Wang, 1945). The facts point to the possibility that in the root-nodule haemoglobin itself, or the processes connected with its synthesis, may be directly linked with nitrogen fixation. On the other hand, haemoglobin may act only indirectly by securing in the root-nodule optimal conditions for oxidative processes with which nitrogen fixation may be associated. Carbon monoxide is known to inhibit symbiotic nitrogen fixation at low pressures, at which it reacts with haemoglobin. Virtanen and Laine (1946) have found methaemoglobin also in root-nodules, and consider that an equilibrium exists there between haemoglobin and methaemoglobin. As a result of experiments on the excretion of nitrogen products by legumes they have made an hypothesis, expressed in the following equations, relating nitrogen fixation to valency changes in haemoglobin. (They make the proviso that the hydroxylamine mentioned in the first equation need not appear directly from the molecular nitrogen but possibly only after a number of intermediate stages.)

Keilin and Smith (1947), however, deny the presence of methaemoglobin as a normal constituent of the root-nodule, and do not support the view that nitrogen fixation involves valency changes in haemoglobin.

Facts obtained by Virtanen and Laine in favour of their view are as follows:

1. Cultures of legumes in sterile sand containing the proper species of *Rhizobia* excrete *l*-aspartic acid and β-alanine, the latter probably arising from the former by decarboxylation. The excretion takes place only when nitrogen is being fixed.
2. A small quantity of oxalacetic oxime is also excreted under the same conditions.
3. Free oxalacetate is formed in pea plants actively fixing nitrogen.
4. Excised nodules, intact or crushed, are able to fix atmospheric nitrogen in presence of oxalacetate but not in its absence.

There have been failures to confirm these phenomena, but Virtanen and Laine's view offers a useful working hypothesis.

NON-SYMBIOTIC NITROGEN FIXATION

Winogradsky in 1893 found that an anaerobic soil organism, *Clostridium pastorianum*, will fix free nitrogen when supplied with glucose, the amount of nitrogen fixed being proportional to the amount of glucose broken down. The fixation of nitrogen is inhibited by the presence of ammonium salts, and this inhibition may be counteracted by an increase in the glucose concentration. Thus the ratio of carbohydrate to combined nitrogen determines the rate of fixation of nitrogen. An interesting feature of this organism is that it loses its power of nitrogen fixation during prolonged cultivation on artificial media, but the power is restored by culture of the organism once again in soil.

Beijerinck in 1901 isolated from soil and mud two aerobic organisms capable of fixing atmospheric nitrogen. They are *Azotobacter chroöcoccum* (the more common form) and *Azotobacter agilis* (the motile variety). Unlike *Clostridium*, *Azotobacter* does not lose its power of fixing nitrogen on prolonged culture on synthetic laboratory media. A striking fact concerning *Azotobacter* is that it requires for its metabolism the presence of traces of molybdenum or vanadium. A positive effect of

$$N_2 + \text{methaemoglobin (Fe'')} \rightleftharpoons NH_2OH + \text{haemoglobin (Fe'')} \quad \ldots\ldots\ldots\ldots (1)$$

$$\underset{\text{(Hydroxylamine)}}{NH_2OH} + \underset{\text{(Oxalacetic acid)}}{COOH.CO.CH_2.COOH} \rightleftharpoons \underset{\text{(Oxalacetic oxime)}}{COOH.C(NOH).CH_2.COOH} + H_2O \quad \ldots\ldots (2)$$

$$COOH.C(NOH).CH_2.COOH \xrightarrow{\text{Reduction}} \underset{\text{(Aspartic acid)}}{COOH.CH(NH_2).CH_2.COOH} \quad \ldots\ldots\ldots (3)$$

molybdenum on the growth of the organism can be observed at a concentration of 1–3 parts in 10^{12}. Burk and Horner have found that molybdenum is not only required for the assimilation of free nitrogen but is necessary for the utilization of combined nitrogen (as of asparagine or nitrate) by this organism. Tungsten will to some extent replace molybdenum. *Azotobacter* and *Clostridium* are apparently the most widely distributed non-symbiotic nitrogen-fixers in soil; they are found also in salt and fresh water, often in association with algae.

In arid soils relatively poor in organic matter, micro-organisms form more than their usual proportion (about 5 per cent.) of the organic matter, and this is chiefly due to the marked development of *Azotobacter* under the alkaline or saline conditions of such soils. In the chestnut soils of south-east Russia, where almost the whole of the organic matter is in the form of micro-organisms, there are up to 900,000,000 *Azotobacter* cells per gram of soil.

The most important single factor influencing nitrogen fixation in soils is the presence of nitrate. With both *Clostridium* and *Azotobacter* the presence of utilizable combined nitrogen diminishes the rate of nitrogen fixation, ammonium or nitrate nitrogen being effective in this way. Inhibition of fixation by *Azotobacter* is complete in the presence of ammonium nitrogen at a concentration of 0·5 mg. of nitrogen per 100 ml. The presence of nitrate or of ammonium salts in the soil also makes legumes resistant to attack by *Rhizobia*, fewer root hairs and nodules being formed. The net result is that, when excess combined nitrogen is available in the soil, little or no fixation of atmospheric nitrogen takes place. The presence of carbohydrates diminishes the effect due to the combined nitrogen.

NITROGEN FIXATION BY *AZOTOBACTER*

Kostytschew and his colleagues (1925) came to the conclusion that the initial process in fixation of nitrogen by cultures of *Azotobacter* is the formation of ammonia, and Winogradsky (1930) discovered that ammonia is formed when *Azotobacter* proliferates on a silica gel in the absence of combined nitrogen. Burk and Horner (1936) opposed the conclusion that ammonia is the first product of nitrogen fixation. They claimed that the ammonia arose in a secondary manner by oxidative deamination of cell organic matter. Winogradsky (1938) pointed out that pure cultures of *Azotobacter* cannot break down organic matter to ammonia, and concluded that the appearance of ammonia in pure cultures of this organism indicates a direct reduction of molecular nitrogen. Burk and Horner (1939), however, still maintain that the ammonia excretion of *Azotobacter* is largely independent of the form of nitrogen supplied (free or combined).

Using a manometric technique which made it possible to study nitrogen fixation by *Azotobacter* in a more quantitative manner than had hitherto been possible, Burk and his colleagues (1930, 1934) have found (1) that the respiration of *Azotobacter*, which is extremely high, is greatest at pressures of oxygen below that in air; (2) that the presence of utilizable combined nitrogen (ammonia or nitrate) diminishes the rate of free nitrogen fixation; (3) that calcium ions (or strontium ions) are necessary for the fixation, and that the substances which immobilize the calcium by forming insoluble salts with it inhibit the nitrogen fixation; and (4) that molybdenum (or, to a less extent, vanadium) has a highly beneficial effect on *Azotobacter* growth in free nitrogen.

Burk and his colleagues have suggested that the following enzyme mechanism is responsible for nitrogen fixation in *Azotobacter*:

$$N_2 + E \rightleftharpoons N_2E \text{ (rapid)},$$
$$N_2E \longrightarrow E + P \text{ (slow)}.$$

E refers to a specific enzyme (*nitrogenase*) which combines reversibly with the nitrogen molecule to form the enzyme-substrate complex N_2E. This breaks down irreversibly into *E* and the products *P*, which corresponds to an increase in *Azotobacter* cells. The total system responsible for fixation has been termed *Azotase*.

AMMONIA FORMATION

It is known that nitrogen compounds in plant residues are decomposed in soil to form ammonia so long as the ratio of carbon to nitrogen in the organic matter does not greatly exceed 10:1.

Proteins and other nitrogenous compounds are broken down in soil by a variety of organisms, the ultimate nitrogenous product being ammonia. Whether the ammonia appears or not depends on the rate of proliferation of organisms in the soil requiring the ammonia nitrogen for their own synthetic operations. If ample utilizable non-nitrogenous material, such as carbohydrate, is present, the ammonia nitrogen will not appear, as it is entirely used for building up fresh bacterial or fungal matter. The amount of nitrogen liberated as ammonia in protein decomposition may be represented as follows:

$$\left\{\begin{array}{c}\text{Nitrogen of}\\ \text{substance}\\ \text{decomposed}\end{array}\right\} - \left\{\begin{array}{c}\text{Nitrogen synthesized}\\ \text{by micro-organisms}\\ \text{for growth}\end{array}\right\} + \left\{\begin{array}{c}\text{Unassimilable}\\ \text{nitrogen}\end{array}\right\} = \left\{\begin{array}{c}\text{Ammonium}\\ \text{nitrogen}\end{array}\right\}$$

Proteins being rich in nitrogen yield ammonia in relatively large amounts. Many organisms are involved in ammonia formation, the mechanisms of which may vary from organism to organism. If the proteins are broken down to amino-acids, these may yield ammonia by the exercise of oxidase systems of either resting or proliferating micro-organisms. Little is known, however, of the modes of decomposition of nitrogenous organic matter in soil.

NITRIFICATION

The metabolic process whereby ammonia and organic nitrogenous material in soil are converted finally to nitrate was shown by Schlossing and Muntz (1877, 1874), from a study of the purification of sewage waters by land filters, to be a biological process. Warington found that soil nitrification is inhibited by application of chloroform and carbon disulphide, and both he and the Franklands (1890) established the fact that nitrification proceeds in two stages, ammonia being oxidized to nitrite and the nitrite to nitrate. Winogradsky (1890, 1891) succeeded in isolating the responsible organisms in pure culture, an achievement of great importance in the study of autotrophic organisms. Organic matter was shown to be unnecessary for the metabolism, and in fact it was stated to be injurious to their development. Warington made it clear that the final fate of nitrogen in the soil is the formation of nitrate. Progress in the study of soil nitrification since the end of last century has been extraordinarily slow. Stevens and Withers (1910) showed that nitrification in soil differs in at least one important aspect from that in the artificial (silica gel) media first elaborated by Winogradsky. They demonstrated that nitrification is inhibited far less in soil by the presence of added organic matter than in laboratory media. Much important work on the behaviour of nitrifying organisms in pure culture in laboratory media has been carried out by Meyerhof (1916, 1917) and others, but the general relations between the results obtained in such culture experiments and those obtained in soil are obscure. Albrecht and McCalla (1937) have summarized the position thus:

> The conditions controlling nitrification in aqueous solution have been studied very specifically. Less definite controls and less refinement in methods have been obtained for studies of this process within the soil. The complexity of a sand, silt, and clay mixture, as soil, prohibits an accuracy great enough to encompass all the various chemical aspects of so delicate a process as nitrification.

Lees and the present writer (1944, 1946) have carried out experiments on the mode of transformation of nitrogen compounds into nitrate in soil, using a new technique with which metabolic events in the soil can be studied with greater accuracy than has been accomplished hitherto. In essence, the attempt has been made to study a soil as though it were a living tissue. Emphasis is given to the changes brought about by the soil *as a whole* under defined experimental conditions, and care is taken that the soil itself is not interfered with throughout the experimental period. The principle is recognized that 'the biological changes taking place in soil are a direct result of the initial chemical stimulus applied to the soil, and are as much a part of the over-all chemical change as the more easily identified metabolic changes themselves' (Lees and Quastel, 1946).

An apparatus is used in the new technique whereby a column of soil (in the form of sieved air-dried crumbs) is perfused with oxygenated or aerated fluid by a circulatory process. This same soil solution is made to percolate through the soil for an indefinite period. The soil perfusate is adequately mixed and aerated and the perfusion is intermittent, so that waterlogging of the soil does not take place. The process is continuous and may be maintained for an indefinite period. The substance whose metabolism is being investigated is dissolved in the perfusion fluid or mixed with the column of soil. Analysis is confined to the constituents of the perfusate, but the soil may be examined after any arbitrary time for analysis of ions adsorbed on it. The apparatus has many advantages for the biochemical study of soil, and with it many aspects of metabolism are as amenable to study in soil as they are in plant or animal tissues. The soil is, in fact, treated as a biological whole, every effort being made to ensure constancy of the environment in which the soil is exercising its metabolic functions.

Experiments with this apparatus confirmed all earlier observations on the biological nature of soil nitrification. Further experiments gave rise to the conclusion that the rate of nitrification of a given quantity of ammonium sulphate is a

function of the degree to which the ammonium ions are adsorbed on, or combined in, the soil in the form of the soil's base-exchange complexes. The greater the amount of adsorption, the faster is the nitrification. This was shown by comparing the rates of nitrification of soils having different amounts of ammonium ions adsorbed upon them. The only tenable explanation of the results was that the adsorbed ammonium ions are those which are preferentially nitrified by the soil organisms. This led to the prediction that the addition of sterile soils to a nitrifying soil would increase its rate of nitrification in proportion to the base-exchange capacities of the added soil. This prediction was verified.

The interpretation of these results is that the nitrifying bacteria grow on the surfaces of the soil crumbs at the sites where ammonium ions are held in base-exchange combination, and proliferate at the expense of such adsorbed ammonium ions. The rate of proliferation becomes proportional, therefore, to the area of soil surface on which ammonium ions are adsorbed or combined, and is thus a function of the base-exchange capacity of the soil.

The fact that proliferation of the nitrifying organism takes place only at those specific sites of the soil surface where the ammonium ions are adsorbed leads to the conclusion that, when all these sites at the surface have been occupied, further growth of the organisms will not occur except to replace cells which have died and disintegrated. Remarkably few living nitrifying cells enter into the soil solution. This leads to the conception of a bacteria-saturated soil; that is to say, a soil where the area of proliferation is limited and cannot be extended owing to full occupancy of available sites for proliferation. Such a soil may be shown to break down substrates, specific for the organisms which enrich the soil, at constant rates. It does not exhibit the familiar logarithmic course of substrate-breakdown which obtains during proliferation. An important use may be made of such a bacteria-saturated soil. It may be made to yield information as to whether any given substance is broken down by the cells which saturate the soil. If an organic nitrogen compound, for example, is broken down and oxidized to nitrate by nitrifying cells, then the course of nitrate formation in a soil saturated with such cells should be linear, and show no initial lag period. If a lag phase does take place, the inference is that the compound in question needs attack by organisms other than the nitrifiers before nitrification can take place. In this way it has been proved that aliphatic amines, which are nitrified in soil, require organisms other than the nitrifiers to effect the initial decomposition.

An interesting new observation has been that while hydroxylamine, at small concentrations, is toxic to the nitrifying organisms and is apparently not nitrified, the presence of sodium pyruvate secures a speedy nitrification of the amine. Pyruvic oxime also undergoes rapid nitrification by a soil enriched with nitrifying organisms. In view of the hypothesis of Virtanen and Laine that oxalacetic oxime is involved in symbiotic nitrogen fixation, the fact that pyruvic oxime undergoes rapid nitrification may throw light on the mechanism whereby ammonia is oxidized to nitrate in nitrifying cells.

Another phenomenon observed by means of the perfusion apparatus is the remarkable bacteriostatic effect of potassium chlorate on the organisms that convert nitrite to nitrate. Small concentrations of chlorates (e.g. $M/10^6$) have the power of preventing the development of *Nitrobacter*, while that of *Nitrosomonas* proceeds unchecked. The result is that when nitrogenous substances are nitrified in soil in the presence of small quantities of chlorates, nitrites but not nitrates accumulate. Potassium chlorate acts as a typical bacteriostatic agent. It does not poison or interfere with the bacterial oxidation of nitrite to nitrate, for with a bacteria-enriched soil the conversion of nitrite to nitrate proceeds at a constant rate uninfluenced by concentrations of chlorate which inhibit proliferation of the organisms involved. Chlorate bacteriostasis may be neutralized by the presence of nitrates, which appear to act specifically. Explanations of these phenomena are still lacking.

LIBERATION OF FREE NITROGEN

The conditions under which this process takes place are not well understood, but it appears to be agreed that the loss of free nitrogen from the soil is greatly encouraged by poor drainage and lack of aeration. It may, however, be of considerable magnitude in well-managed cropped soils and probably also in lands still in a virgin condition.

Numerous heterotrophic organisms are known to be able to break down nitrates and nitrites with liberation of nitrogen (or oxides of nitrogen), but their quantitative significance in soils has not yet been assessed.

Nitrites react, of course, in acid or slightly acid solution with urea and amines to liberate nitrogen. It is possible, though it seems unlikely, that this chemical mechanism may form a major means by which free nitrogen leaves the soil.

Book reviews

IDENTIFICATION OF ORGANIC COMPOUNDS

Characterization of Organic Compounds, *by F. Wild. Pp.* 306. *University Press, Cambridge.* 1947. 18*s. net.*

Senior students of organic chemistry and research workers are frequently confronted with the problem of identifying organic compounds and of preparing derivatives of new substances which may serve for their future characterization. This is especially necessary in the case of liquids. Suitable derivatives should be solids with well-defined melting-points, capable of ready preparation in a state of purity. Many methods are well known, but numerous new reagents and procedures have been introduced in recent years, and if a satisfactory derivative is not readily preparable the chemist may spend much time in searching the literature.

This excellent book gives all the important general methods to the end of 1943, with full practical details for the proper preparation of the derivatives and frequently of the reagents themselves. It includes many references to original, mainly recent, papers and tables of melting-points. The arrangement of the book is according to the type of compound to be characterized. The headings include hydrocarbons, halogen and hydroxy compounds, aldehydes, ketones, acids and derivatives, and amines and various other nitrogen-containing compounds. Author and full subject indexes are provided.

The need for such a volume has long been pressing, and Dr Wild's book is destined to find an honoured place on the shelves of all organic laboratories.

W. BAKER

MATHEMATICS IN PHYSICS

Methods of Mathematical Physics, *by Harold Jeffreys and Bertha Swirles Jeffreys. Pp.* x + 679. *University Press, Cambridge.* 63*s. net.*

No one is better qualified to write a book on the methods of mathematical physics than the versatile Harold Jeffreys, now Eddington's successor at Cambridge. Here, in collaboration with Mrs. Jeffreys, herself an experienced quantum physicist, he gives a complete account of all the methods used in at least two distinct branches of theoretical physics. The reputation of the authors is in itself sufficient to recommend the book, but if anyone wants to have what is easily the most complete account of the techniques now so commonly employed he cannot possibly do better than get this volume. It is a mammoth book of nearly seven hundred pages, including as two of its chapters the well-known tracts on operational methods and Cartesian tensors, now out of print. It is not everyone's meat, but those who suddenly find themselves needing a particular technique (e.g. complex integration or the use of Fourier integrals) will get what they want here; and as they turn over the pages they will be grateful for a high standard of rigour, no loose statements, and withal a certain racy sense of humour. They will realize the quite astonishing way in which different branches of physics are unified by virtue of the mathematical methods used in discussing them. This is a volume in the best Cambridge 'big blue book' tradition.

C. A. COULSON

ANIMAL HUSBANDRY

The Breeding of Farm Animals, *by Chapman Pincher. Pp.* 149, *with* 7 *plates and* 65 *figures. Penguin Books, London.* 1946. 1*s. net.*

While plant breeding is usually done by experts, the breeding of animals is carried out by the individual farmer. This being so, it is all the more remarkable that few, if any, courses on the science of animal breeding are included in present agricultural courses at colleges and farm institutes. This book, which sets out in simple language the principles of scientific animal breeding, should therefore supply much-needed information. Chapters on genes, Mendelism, dominance, and sex linkage are well illustrated by diagrams which will greatly assist those knowing nothing of the subject.

In the introduction the author points out that in pre-war years the annual value of the edible products of British agriculture was £250,000,000, and that of this total 60 per cent. was contributed by the industry of animal husbandry. Improvements in the efficiency of animal production would therefore be to our material advantage. In a chapter on systems of breeding the uses of inbreeding, out-crossing, and grading-up are discussed, and stress is laid on the necessity for keeping records of the performance of offspring.

J. HAMMOND

AGRICULTURAL BOTANY

Principles of Agricultural Botany, *by Alexander Nelson. Pp. xviii* + 556. *Thomas Nelson & Sons Limited, London.* 1946. 35*s. net.*

The more we live in towns, the greater are our demands on the country: we all realize now that the proper use of the resources of the plant kingdom is a vital necessity. This scientific management of crop plants is a profession still in its infancy but destined for rapid expansion. For the training of recruits to this profession Dr Alexander Nelson has prepared a textbook to stand beside the *Agricultural Botany* of John Percival. In the light of his wide agricultural knowledge and his teaching experience in the University of Edinburgh, Dr Nelson puts mastery of technological details second to a grasp of principles applicable to a diversity of crops.

His first section, of 229 pages, treats of plant morphology and anatomy, furnishing among other illustrative examples five comprehensive tables for the identification of trees in winter, of common legumes, and of grasses. The second section, of 202 pages, describes the principles of plant physiology, concluding with a chapter on pasture. Section 3 introduces in 67 pages the subject of weeds, insect pests, and plant diseases. The volume concludes with a 34-page section on heredity, evolution, and classification.

Judged by wartime standards, the production has an air of refreshing opulence. There are 145 full-page plates, including 17 in colour, and 182 text figures. Almost without exception the illustrations are novel and apposite. The text is set in a well-spaced 11-point fount, and the proof-reading has been efficient.

Dr Nelson's work cannot fail to be appreciated in this country: an allocation of copies would be even more welcome overseas, where there is a pressing need for botanical textbooks with a background of British Commonwealth experience.

R. W. MARSH

IPATIEFF'S LIFE-STORY

The Life of a Chemist: Memoirs of V. N. Ipatieff, *edited by X. J. Eudin, H. D. Fisher, and H. H. Fisher, translated by V. Haensel and Mrs. R. H. Lusher. Pp. xvi + 658, with 7 plates. Stanford University Press, California; Geoffrey Cumberlege, London.* 1946. 33*s.* 6*d. net.*

Vladimir Nikolaevich Ipatieff is assured of a permanent place among the great Russian chemists. His long career, so full of variety, of light and of shade, has always borne the mark of genius. Born in 1867, Ipatieff achieved distinction in Tsarist Russia. He became Professor Ordinarius at Mikhail Artillery Academy and lecturer in the University of St Petersburg in 1902, and two years later rose to the military rank of colonel. In 1910 he was promoted to major-general, and in 1916 he was elected into the Russian Academy of Sciences. Throughout this period and for twelve years after the Revolution he cultivated with outstanding success and vigour a fruitful field of chemical research, dealing particularly with catalysis at high temperatures and pressures. As an authority on explosives and chemical warfare he became an indispensable figure in Russia during the first world war. Under the early Soviet regime he took a leading and forceful part in the development of Russian chemical industries. For all this work his country owes him a great debt. Ipatieff tells in this book the story of his life in simple and unadorned terms. He describes his scientific work only in bare outline, and fills in the background with a rich and variegated sketch of Russian life and personalities, charged with much detail of historical value. Although he held aloof from politics and tried to do his best for his country, this illustrious chemist found himself obliged in 1930 to leave for ever, as he says, 'my beloved country, my beloved Leningrad, and those laboratories in which I had spent the happiest hours of my life.' Fortunately he was able to continue his scientific work in America, as professor in the Northwestern University and Director of Research to the Universal Oil Products Company.

JOHN READ

PLASTICS FOR INDUSTRY

Plastics for Production, *by Paul I. Smith. Pp.* 216. *Chapman and Hall Limited, London.* 1946. 15*s. net.*

Today we are only too frequently told that we live in an age of plastics, and in many respects this is true. During the last ten years plastics have been playing an increasingly important part as raw materials for modern industry. Where wood or metal or ceramics had previously served we now find plastics doing the job instead.

In certain fields it is inevitable that plastics must supplant these traditional materials. But the real future of plastics lies in their use as new substances in their own right. They are taking their place alongside the established products to provide us with a more versatile and extended range of raw materials.

Unfortunately, plastics are suffering today from a surfeit of publicity, and have been attributed with almost limitless potentialities. Plastics, like other materials, have their limitations: they cannot do everything. In *Plastics for Production* we find what they can do and what they cannot. Paul I. Smith has written this book for the engineer who is looking to plastics for a raw material suitable for his work. All the common plastics are here, and their properties are discussed critically and dispassionately. For anyone in search of information that will enable him to discriminate between the multitude of plastics available to him this book should prove invaluable. J. G. COOK

DETERMINATION OF HYPER-FINE STRUCTURE

High Resolution Spectroscopy, *by S. Tolansky. Pp.* 291 + 4 *plates. Methuen and Company Limited, London.* 1947. 21*s. net.*

This is a practical account of the techniques rather than the results of high-resolution spectroscopy. A detailed consideration of the theory and performance of the Fabry-Perot and Lummer plate interferometers and of the echelon grating assumes a central position in the book, and occupies about half its length. The writing here, as elsewhere, is clear and authoritative.

A notable section of the book (and one which this reader must confess to have appreciated most) deals with light sources (67 pp.). These chapters contain first-hand, critical accounts of the experimental arrangements used for exciting spectra—the emphasis being, of course, on fine-line atomic excitation—which will prove of great interest, not only to spectroscopists in general but to workers in allied fields. Much of the information given here was previously to be found at best in scattered papers in the literature, and often only as a verbal laboratory tradition. Moreover, in times when the tendency can be discerned for research to become subordinated to apparatus-making, Dr Tolansky's descriptions of simple, elegant devices which work, and which provide answers to crucial experiments, are stimulating. A good book.

R. F. BARROW

PENICILLIN

Penicillin, its Properties, Uses and Preparations. *Pp. vi* + 199. *The Pharmaceutical Press, London.* 1946. 10*s.* 6*d. net.*

The production of penicillin is now so great that in almost all parts of the world the drug is freely available for all who need it remedially, and for research purposes. As a result large numbers of pharmacists, physicians, research workers, and others require to know the best methods of handling it. All the necessary information will be found in this excellent little book published by direction of the Council of the Pharmaceutical Society of Great Britain. Although this is essentially a practical handbook for pharmacists, the compilers have wisely recognized that the intelligent use of penicillin depends upon a general knowledge of all its properties. Consequently an account is given of the history of penicillin, its commercial manufacture, and other aspects of the subject not directly related to its pharmaceutical use. Although these less relevant aspects are necessarily dealt with quite briefly, there is an extensive bibliography for those who wish to read more widely. The sections on legislation relating to penicillin describe British and American practice, but this is similar to that followed in many other countries.

An account is given of the indications for the clinical use of penicillin and of methods of dispensing and storing solutions, ointments, creams, lozenges, and other pharmaceutical preparations containing the drug. There is also a description of the cylinder plate, serial dilution, and other standard methods of assaying penicillin.

This book can be strongly recommended, and if revised from time to time it should remain a standard work for many years.

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Notes on contributors

ARTHUR HOLMES,
D.Sc., A.R.C.S., F.R.S.,

Is Regius Professor of Geology and Mineralogy in the University of Edinburgh. He was born in 1890 and was educated at Gateshead Secondary School and the Imperial College of Science and Technology, London. His first research—on the accumulation of lead in radioactive minerals, carried out under the guidance of Lord Rayleigh—was interrupted by a period of geological exploration in Mozambique, after which he returned to Imperial College as a Demonstrator in Geology (1912). In 1920 he went to Burma as chief geologist of the Yomah Oil Company. In 1924 he was appointed Reader of Geology, and three years later Professor, in the Durham Division of the University of Durham, which post he continued to hold until 1933. He served as Exchange-Professor at Basle in 1931, and as Lowell Lecturer at Boston in 1932. His chief scientific interests are problems of petrology and petrogenesis, and the applications of radioactivity to geology. Besides having made many important contributions on these subjects to scientific journals, he is the author of several well-known books. He has been an active member of the National Research Council Committee on the Measurement of Geologic Time since its foundation. In 1940 he was awarded the Murchison Medal of the Geological Society of London.

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Is a graduate of Glasgow University. He joined the Geological Survey in 1909, became a district geologist in 1919, and in 1924 was appointed Assistant Director in charge of survey activities in Scotland. This post he held until his retirement in 1945. In the course of his career he has made many contributions to official publications and to scientific periodicals, mainly in the sphere of stratigraphical, economic, and glacial geology. He achieved conspicuous success in organizing increasingly detailed work on Scottish mineral deposits and in promoting close collaboration with the mining and industrial community. His services to geology have been recognized by the award of the Murchison Medal of the Geological Society of London in 1941, and of the Clough Memorial Medal for Geology of the Edinburgh Geological Society (1942–4). Dr Macgregor is a past president of the Geological Societies of Glasgow and of Edinburgh, a past vice-president of the Geological Society of London, and editor of the *Transactions of the Glasgow Geological Society*. He is at present president of the Geological Section of the British Association for the Advancement of Science.

L. M. ANGUS-BUTTERWORTH,
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Was educated at the Department of Glass Technology, University of Sheffield, and at the Universities of Manchester and Toulouse. He is a director of the Newton Glass Works, Manchester, founded in 1795. A Fellow of the Society of Glass Technology, he is at present chairman of the North-East Section. He has written a book, *The Manufacture of Glass*, to be published this year by Sir Isaac Pitman & Sons Limited. He is a past chairman of the Manchester Literary and Philosophical Society and *Membre Honoraire de l'Institut Historique de France*. He is also a governor of the Royal Scottish Corporation and the Royal Caledonian Schools.

D. STARKIE,
M.Sc., Ph.D.,

Was born in 1904 and was educated at Netherthorpe Grammar School, Staveley, and the University of Sheffield, where, after graduating, he served for some time on the teaching staff. He then took charge of the Optics and Illuminating Laboratories of the Metropolitan-Vickers Electrical Company at Manchester, and later became chief physicist at the Wear Glass Works (Sunderland) of J. A. Jobling and Company. Upon his appointment as head of the Physical Section of the Plastics Division's Research Department of Imperial Chemical Industries, he initiated investigations on optical plastics. The research developed along original lines and by 1945 had become so promising that a separate optical department was established with Dr Starkie in charge. Dr Starkie is well known throughout the optical industry in Britain, and is a member of the council of the British Scientific Instrument Research Association.

M. SCHOFIELD,
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Was educated at Bradford Grammar School and at Magdalen College, Oxford, where he was an Open Scholar in Natural Science from 1920 to 1924. After leaving Oxford he became senior science master at Worksop College, and is now Responsible Lecturer in Chemistry at Wolverhampton and Staffordshire Technical College. He is author of a considerable number of studies, both on biographical subjects and chemical history, published in *Chemistry and Industry*, *Discovery*, *Science Progress*, etc. He has also contributed reviews of industrial progress to supplements of *The Times* and to various journals of chemical industry and technology. For some years he studied the wood distillation industry, and published original papers on the subject in the *Transactions of the Institute of Chemical Engineers* and other technological journals.

J. H. QUASTEL,
M.A., D.Sc., Ph.D., F.R.I.C., F.R.S.,

Was director of the Soil Metabolism Unit at University College, Cardiff, until this year, but has now been appointed Professor of Biochemistry at McGill University and Associate Director of the Institute of Cell Metabolism, Montreal. He was born in 1899 and was educated at the Central Secondary School, Sheffield, the Imperial College of Science, London, and Trinity College, Cambridge. He served in the war of 1914–18 and on demobilization studied chemistry at the Royal College of Science, London, afterwards carrying out research in the School of Biochemistry, Cambridge (1921–9). His work there was mainly devoted to the biochemistry of bacteria. In 1929 he was appointed Director of Research at Cardiff City Mental Hospital and carried out researches on the biochemistry of the central nervous system and the mechanism of the action of narcotics and benzedrine. He was the first to show the synthesis of acetylcholine in isolated brain tissue and to study fat metabolism in isolated surviving tissue slices. He has worked on enzyme systems in brain and other tissues and has correlated clinical with laboratory results in the investigation of mental disorder and treatment. In 1941 he was appointed director of the Agricultural Research Council Unit of Soil Biochemistry.

Made and printed in Great Britain at THE KYNOCH PRESS, *Witton, Birmingham, 6, for the publishers,*
IMPERIAL CHEMICAL INDUSTRIES LIMITED, *London*, S.W. 1